THE MATHEMATICAL GAZETTE

EDITED BY
W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF
F. S. MACAULAY, M.A., D.Sc.
AND
Prof. E. T. WHITTAKER, Sc.D., F.R.S.

LONDON
G. BELL & SONS, LTD., PORTUGAL STREET, KINGSWAY, W.C. 2.
AND BOMBAY

Vol. XIII., No. 191. DECEMBER, 1927. 2s. 6d. Net.

CONTENTS.

	PAGE
Term-by-Term Integration of Infinite Series. Prof. H. S. Carslaw, Sc.D.,	437
The Equation of Activity in Dynamics and the Principle of Zero Activity in Statics. Prof. H. R. Hassé, M.A., D.Sc.,	442
The Laws of Motion; a Dialogue. G. H. Lester, M.A.,	449
Mathematical Notes (882–885). W. J. Langford, B.A.; H. Lob, M.A.; J. Peacock, M.A.; C. O. Tuckey, M.A.,	455
Reviews. Major C. H. Chepmell, R.A.; Prof. F. R. W. Hunt, M.A.; W. Hope-Jones, B.A.; J. E. Lennard-Jones, Ph.D.; Prof. E. H. Neville, M.A.; H. E. Piggott, M.A.; A. Robson, M.A.; E. F. Sheppard, M.A.,	460
Gleanings Far and Near (473–478),	441
Books Received, etc.,	i

The Mathematical Association.

THE ANNUAL MEETING will be held at the London Day Training College, Southampton Row, London, W.C. 1, on *Thursday, 5th January*, 1928, at 5.30 p.m. (Advanced Section); and on *Friday, 6th January*, 1928, at 10 a.m. and 2.30 p.m. (Ordinary Meeting).

Intending members are requested to communicate with one of the Secretaries. The subscription to the Association is 15s. per annum, and is due on Jan. 1st. It includes the subscription to "The Mathematical Gazette."

Change of Address should be notified to a Secretary. If Copies of the "Gazette" fail for lack of such notification to reach a member, duplicate copies can be supplied only at the published price.

G. Bell & Sons

ELEMENTARY TRIGONOMETRY

By C. V. DURELL, M.A., Senior Mathematical Master, and R. M. WRIGHT, M.A., Second Master, Winchester College. 5s. Also in 3 parts. 2s. each. Parts I. and II. are available bound together. 3s. 6d.

"This excellent textbook contains everything required for Matriculation and School Certificate Examinations. . . . . We recommend teachers of mathematics to examine the book with a view to its adoption."—*The A.M.A.*

"This is a clearly written and interesting book. . . . . We thoroughly recommend it."—*School Science Review.*

THE ELEMENTS OF THE CALCULUS

By W. P. MILNE, M.A., D.Sc., Professor of Mathematics, Leeds University, and G. J. B. WESTCOTT, M.A., Head Mathematical Master, Bristol Grammar School. 3s.

Specially designed for students who require in small compass a good working knowledge of the Calculus. The examples are mostly drawn from Geometry. The course covers, with illustrative examples, the differentiation and integration of powers of x, trigonometrical functions, logarithmic and exponential functions, together with sufficient treatment of the more elementary manipulative processes. The student will then be in a position to use the Calculus freely and intelligently in studying the problems of Geometry, Mechanics, Physics, Chemistry, Statistics, &c.

ELEMENTS OF THE MATHEMATICAL THEORY OF LIMITS

By J. G. LEATHEM, Sc.D., late Fellow of St. John's College, Cambridge. Demy 8vo. 14s. net.

Gives, in an eminently practical and readable form, a sound introduction to University mathematics. A large number of examples is provided, and the solutions are indicated.

"Will be found of the greatest value in enabling the student from the outset to acquire correct and well-defined ideas on this important subject. It is obvious that behind the book lies a large amount of teaching experience."
Journal of Education.

MATHEMATICAL TABLES

Compiled by C. V. DURELL, M.A. 40 pp. Limp cloth. 9d.

"A complete set of four figure tables containing all that the average secondary student is likely to require, clearly printed, bound in a stout cover and costing ninepence, is something of an achievement in post war times."
Journal of Education.

Mathematics Catalogue post free on request.

YORK HOUSE, PORTUGAL ST., LONDON, W.C.2

THE

MATHEMATICAL GAZETTE.

EDITED BY

W. J. GREENSTREET, M.A.

WITH THE CO-OPERATION OF

F. S. MACAULAY, M.A., D.Sc., AND PROF. E. T. WHITTAKER, Sc.D., F.R.S.

LONDON:

G. BELL AND SONS, LTD., PORTUGAL STREET, KINGSWAY

AND BOMBAY.

VOL. XIII. DECEMBER, 1927. No. 191.

TERM-BY-TERM INTEGRATION OF INFINITE SERIES.

BY PROF. H. S. CARSLAW, Sc.D.

§ 1. The English student of Mathematics finds in his text-books a proof that the uniform convergence of a series of continuous * functions is a sufficient condition for term-by-term integration. He knows that there are convergent series which cannot be integrated term-by-term,† and that there are series, not uniformly convergent in the interval of integration, which can be thus integrated.‡ The last remark applies particularly to the Fourier's Series of an ordinary function with discontinuities.§

But when he requires a proof of sufficient conditions for term-by-term integration, other than the standard condition of uniform convergence, he is in difficulty. If he turns to Hobson's *Theory of Functions* ‖ he finds that the results are proved for Lebesgue Integrals. This applies also to most of the papers of W. H. Young, often quoted in this connection.¶ It is true that, when $f(x)$ is integrable in the ordinary sense (*i.e.* when Riemann's definition is used), it is also integrable according to Lebesgue's definition, and the integrals agree. But, of course, a function can have a Lebesgue integral and not a Riemann integral. So, with certain modifications, the conditions for term-by-term integration, to be found in the places named above, are applicable to ordinary integration. But, while a careful numerical treatment of the definite integral on Riemann's lines can, and, I believe, should be included in the course of the average student of the Calculus, the modern theory seems

* As a matter of fact if the functions are integrable and the series converges uniformly, the sum is integrable and term-by-term integration is allowable. But this is much harder to prove than the standard theorem about a series of continuous functions. Cf. Hobson, *Theory of Functions of a Real Variable*, i. (2nd edit. 1921) 446.

† When $S_n = 2n^2xe^{-n^2x^2}$, $\lim s_n = 0$ and $\int_0^x \lim s_n\,dx = 0 \neq \lim \int_0^x s_n\,dx$.

Throughout this paper s_n or $S_n(x)$ stands for the sum of the first n term of a series.

‡ When $s_n = \dfrac{nx}{1+n^2x^2}$, $\lim s_n = 0$ and $\int_0^1 \lim s_n\,dx = 0 = \lim \int_0^1 s_n\,dx$.

Again $\int_0^1 \dfrac{dx}{1+x} = \lim \int_0^1 (-1)^r x^r\,dx$

§ See below, § 2, concluding sentence.
A simple proof is given in de la Vallée Poussin's *Cours d'Analyse Infinitésimale*, 2 (4th ed. 1922), 106, that the Fourier's Series of a function which is absolutely integrable can always be integrated term-by-term, even when the series itself does not converge.

‖ Hobson, *loc. cit.* 2 (2nd ed. 1926), 303.

¶ Cf. *e.g. Proc. London Math. Soc.* (2), 9 (1910), p. 316 and p. 464.

to me a matter for the specialist in the Theory of Functions of a Real Variable. At any rate the idea of the measure of a set of points, upon which it rests, is a difficult one ; and the most useful condition for term-by-term integration, other than the standard one of uniform convergence, can be obtained without using that concept.

In this note I suggest an addition to the subject-matter of the usual course on the Calculus. In my own class I introduce Theorem I. after the customary proof of term-by-term integration when the series converges uniformly. It offers no difficulty and is useful in practice. It is a special case of Theorem II., the proof of which is harder and suitable only for more advanced students. This second theorem is often ascribed to Osgood,* but it was first proved by Arzela.† The simpler discussion given here is based on papers of Bieberbach‡ and Landau,§ now nearly ten years old, which do not seem to have been noticed by English writers. The theorem on sets of intervals required in the proof is to be found in the final section.

§ 2. Theorem I. *Let the integrable function $f(x)$ be the sum of the series of integrable functions $u_1(x)$, $u_2(x)$, ... and let this series converge uniformly in $a \leqq x \leqq b$, except for a finite number of sub-intervals, the sum of whose lengths can be made less than any given positive number. Also let there be a positive number M such that $|S_n(x)| < M$, for every x in (a, b) and for every positive integer n.*

Then $$\int_a^b f(x)\,dx = \lim \int_a^b S_n(x)\,dx = \lim \sum_1^n \int_a^b u_r(x)\,dx.$$

Let the arbitrary positive number ϵ be chosen, and let the sub-intervals in which the uniform convergence fails be (a_1, b_1), (a_2, b_2) ... (a_k, b_k), where

$$\sum_1^k (b_r - a_r) < \epsilon/4M.$$

Then we have

$$(1)\quad \int_a^b f(x)\,dx - \int_a^b S_n(x)\,dx = \left(\int_a^b - \sum_1^k \int_{a_r}^{b_r}\right)(f(x) - S_n(x))\,dx + \sum_1^k \int_{a_r}^{b_r} (f(x) - S_n(x))\,dx.$$

Now $$|S_n(x)| < M.$$

Therefore $$|f(x)| = |\lim S_n(x)| \leqq M.$$

And $$|f(x) - S_n(x)| < 2M.$$

But the series converges uniformly in (a, b), except for the sub-intervals (a_1, b_1), (a_2, b_2), ... (a_k, b_k).

Thus there is a positive integer ν, such that

$$|f(x) - S_n(x)| < \epsilon/2(b-a)$$

when $n \geqq \nu$, the same ν serving for all values of x in the part of (a, b) which remains when (a_1, b_1), (a_2, b_2) ... (a_k, b_k) are removed.

It follows from (1) that

$$\left|\int_a^b f(x)\,dx - \int_a^b S_n(x)\,dx\right| < \{\epsilon/2(b-a)\} \times (b-a) + (\epsilon/4M) \times 2M < \epsilon, \quad \text{when } n \geqq \nu.$$

Therefore $$\int_a^b f(x)\,dx = \lim \int_a^b S_n(x)\,dx = \lim \sum_1^n \int_a^b u_r(x)\,dx.$$

---

* Cf. *e.g.* Bromwich, *Theory of Infinite Series* (2nd ed. 1926), footnote on p. 134, and Ex. 22 on p. 144. I wonder if an elementary proof of the first part of this example is possible. For the second part, when the functions are integrable, see below, §§ 3, 4.

† *Rend. Acc. Linc.* (4), 1 (1885) 537, and, more fully, in *Mem. Ist. Bologna* (5), 8 (1900), 723. Osgood, independently of Arzela, gave the case for continuous functions in *Amer. Journal of Math.* 19 (1897), 155.

‡ Bieberbach, "Über einen Osgoodschen Satz aus der Integralrechnung," *Math. Zeitschrift*, 2 (1918), 155.

§ Landau, "Ein Satz über Riemannsche Integrale," *ibid.* p. 350.

With a slight alteration in this argument it is easy to show that under the conditions named above

$$\int_a^x f(x)\,dx = \lim \sum_1^n \int_a^x u_r(x)\,dx, \text{ when } u \leqq x \leqq b,$$

and that the series of integrals on the right converges uniformly to $\int_a^x f(x)\,dx$ in (a, b).

Again, when the arbitrary function $f(x)$ is of bounded variation in $(-\pi, \pi)$, the sum of n terms of its Fourier's Series satisfies the condition $|S_n(x)| < M$.*

If, in addition, the discontinuities of $f(x)$ can be enclosed in a finite number of intervals, the sum of whose lengths is less than any given positive number, the theorem just proved shows that its Fourier's Series can be integrated term-by-term.†

§ 3. Theorem II. *Let the integrable function $f(x)$ be the sum of the series of integrable functions $u_1(x)$, $u_2(x)$, ... and let there be a positive number M such that $|S_n(x)| < M$, for every x in (a, b) and for every positive integer n.*

Then

$$\int_a^b f(x)\,dx = \lim \int_a^b S_n(x)\,dx = \lim \sum_1^n \int_a^b u_r(x)\,dx.$$

(i) Let $f(x) = 0$. We have to prove that $\lim \int_a^b S_n(x)\,dx = 0$. If this is not the case, there must be a positive number ϵ such that

$$\left| \int_a^b S_n(x)\,dx \right| > 2\epsilon(b-a),$$

for an infinite number of values of n, say $n_1, n_2, \ldots$.

Let n be one of these values.

Then there must be a mode of division of (a, b) into a finite number of partial intervals $i_1, i_2, i_3 \ldots i_k$, such that the sum s, with the ordinary notation for Riemann's integral, for this mode of division satisfies $|s| > 2\epsilon(b-a)$.

Let the sum of the lengths of the intervals i_r in this mode of division for which the absolute value of the lower bound m_r of $S_n(x)$ is greater than or equal to ϵ be j.

Then we have

$$\left| \sum_{(m)<\epsilon} (m_r i_r) + \sum_{(m)\geqq\epsilon} (m_r i_r) \right| > 2\epsilon(b-a),$$

the summations being taken over the intervals i_r in which $|m_r| < \epsilon$ and $|m_r| \geqq \epsilon$ respectively.

It follows that

$$\sum_{(m)<\epsilon} |m_r|\, i_r + \sum_{(m)\geqq\epsilon} (m_r)\, i_r > 2\epsilon(b-a).$$

Therefore $$\epsilon(b-a) + Mj > 2\epsilon(b-a),$$

and $$j > \epsilon(b-a)/M.$$

Thus we have for each of the numbers $n_1, n_2, \ldots$ a set of points, consisting of the points of a finite number of not-overlapping intervals in (a, b), the sum of the lengths of the intervals in each set being greater than a definite positive number. There must ‡ be at least one point x which lies in an infinite number of the intervals.

For this value of x we have $|S_n(x)| \geqq \epsilon$ for an infinite number of values of n.

---

* Cf. *e.g.* Hobson, *loc. cit.* 2 (2nd ed. 1916), p. 512.
This result can also be deduced from the Gibbs' Phenomenon in Fourier's Series. Cf. my *Fourier's Series and Integrals*, ch. ix. (2nd ed. 1921).

† See also footnote §, p. 1.

‡ The proof of the theorem on sets of intervals which is used here will be found in the next section.

But this is impossible, since we are given that $\lim S_n(x)=0$ for every x in (a, b).

Thus we have shown that, under the conditions stated, when the sum of the series vanishes, the sum of the series of integrals also vanishes.

(ii) Let $f(x) \geqq 0$.

Write $$v_1(x)=u_1(x)-f(x), \quad v_2(x)=u_2(x), \ldots .$$

Then $$\sum_1^\infty v_r(x)=0 \quad \text{and} \quad \left|\sum_1^n v_n(x)\right| < 2M,$$

for every x in (a, b) and for every positive integer n.

Thus by (i), $$\lim \int_a^b \sum_1^n v_n(x)\,dx=0.$$

Therefore $$\int_a^b f(x)\,dx=\lim \sum_1^n \int_a^b u_r(x)\,dx.$$

§ 4. *If there be an infinite number of sets of intervals in a finite segment, each set consisting of a finite, or a countably infinite, number of not-overlapping open intervals, such that the sum of the lengths of the intervals in each, or the limit of that sum, is greater than some fixed positive number C, then there is at least one point which lies in an infinite number of the intervals.**

Let $I_1, I_2, \ldots$ be the given sets, each consisting of a finite, or a countably infinite, number of not-overlapping open intervals in (a, b), the sum, or the limit of the sum, of the lengths of the intervals in each set being greater than C.

Let E be the set made up of every point belonging to one or more of the given sets; that is, $E=\sum_1^\infty I_r$.

Then E is composed of a finite or countably infinite set of not-overlapping open intervals, the sum of their lengths being greater than C, for the intervals of $I_1, I_2, \ldots$ are contained in E.

Now take positive numbers $E_1, E_2, \ldots$ such that $\sum_1^\infty E_r < \frac{1}{2}C$.

Choose from E a finite number of its intervals, so that the sum of the lengths of those discarded is less than $\frac{1}{2}E_1$, and close these intervals at both ends, the total length cut off from them being less than $\frac{1}{2}E_1$.

The set of points of this set of closed not-overlapping intervals, finite in number, is denoted by E_1, and the sum of the lengths of its intervals is greater than $C-E_1$.

Now all the points of this closed set are interior points of one or other of the infinite set of intervals of $I_1, I_2, \ldots$.

Therefore by the Heine-Borel Theorem we can obtain all the points of E_1 from a finite number of these intervals.

Suppose intervals from some or all of $I_1, I_2, \ldots I_{n_1}$ are used in this process.

Then $\sum_{n_1+1}^\infty I_r$ is an open set, and consists of a finite or countably infinite set of not-overlapping open intervals, the sum of their lengths being greater than C.

But E_1 was obtained by cutting off from $\sum_1^\infty I_r$ lengths in all $< E_1 < \frac{1}{2}C$.

Therefore part of $\sum_{n_1+1}^\infty I_r$ is left in E_1. This part is denoted by E', and consists of a finite or countably infinite set of not-overlapping intervals in (a, b), the sum of the lengths of these intervals being greater than $C-E_1$.

Then we take E' and treat it as E was treated. We discard a part of it (the total length of the part discarded being less than $\frac{1}{2}E_2$), and we close at

* This theorem was first proved by Arzela, *loc. cit.* The proof given here is due to Bieberbach, *loc. cit.* The theorem is a special case of a more general theorem in sets of points due to W. H. Young, to be found in Hobson's *Theory of Functions*, 2 (2nd ed. 1926), § 136, see especially p. 177.

both ends the finite number of intervals left, cutting off in all from these a length less than $\frac{1}{2}E_2$.

This closed set is denoted by E_2. It consists of a finite number of closed not-overlapping intervals, the sum of their lengths being greater than $C-E_1-E_2$.

Now all the points of this closed set are interior points of one or other of the infinite set of intervals $I_{n_1+1}, I_{n_1+2} \dots$.

Thus we can obtain all the points of E_2 from a finite number of these intervals.

Suppose that intervals of some or all of $I_{n_1+1}, I_{n_1+2} \dots I_{n_2}$ are used in this process.

Then $\sum_{n_2+1}^{\infty} I_r$ is an open set and consists of a finite or countably infinite set of open not-overlapping intervals, the sum of their lengths being greater than C.

But in forming E_2 we cut off from E' a total length less than E_2, and in forming E' we cut off from $\sum_{n_1+1}^{\infty} I_r$ a total length less than E_1. Therefore in forming E_2 we cut off from $\sum_{n_1+1}^{\infty} I_r$ a total length less than $E_1+E_2 < \frac{1}{2}C$.

Thus part of $\sum_{n_2+1}^{\infty} I_r$ is left in E_2. We denote this part by E''. It consists of a finite or countably infinite set of not-overlapping intervals, the sum of their lengths being greater than $C-E_1-E_2$. Then we take E'' and proceed as before, obtaining the closed set E_3, the sum of the lengths of its intervals being greater than $C-E_1-E_2-E_3$.

We have thus obtained sets $E_1, E_2, E_3, \dots$, each set composed of a finite number of closed not-overlapping intervals, the sum of the lengths of the intervals in each being greater than $\frac{1}{2}C$, since $\sum_{1}^{\infty} E_r < \frac{1}{2}C$.

Also E_2 is a component of E_1; E_3 of E_2 and E_1; etc.

We now show that there is at least one point which belongs to an infinite number of the sets $E_1, E_2, E_3, \dots$.

Consider the finite number of intervals of E_1. In at least one of these intervals there must be intervals of an infinite number of $E_2, E_3, \dots$.

Suppose i_1 is an interval of E_1 with this property, and that it contains intervals of $E_2, E_5, E_8, E_{15}, E_{21}, E_{24}, \dots$.

There are intervals of E_2 in i_1. In at least one of these there must be intervals of an infinite number of $E_5, E_8, E_{15}, \dots$, say of $E_8, E_{15}, E_{24}, \dots$.

Let i_2 be this interval.

We obtain in this way an infinite set of intervals $i_1, i_2, i_3, \dots$ such that each contains the following one.

For such a set there must be at least one point belonging to all the intervals.

Thus we have shown that there must be at least one point which belongs to an infinite number of the sets $E_1, E_2, E_3, \dots$. But all the points of E_1 are contained in one or other of the sets $I_1, I_2, \dots I_{n_1}$; and all the points of E_2 are contained in one or other of $I_{n_1+1}, I_{n_1+2}, \dots I_{n_2}$; and so on. It follows that this point must belong to an infinite number of the sets $I_1, I_2, \dots$.

Sydney, Australia, *Feb.* 1927. H. S. CARSLAW.

GLEANINGS FAR AND NEAR.

473. Mathematical knowledge is common enough but mathematicians are rare.—J. C. de la Faille.

474. Newton looked upon geometry as no further useful than it directs us how to make experiments and observations, and draw consequences from them when made.

THE EQUATION OF ACTIVITY IN DYNAMICS, AND THE PRINCIPLE OF ZERO ACTIVITY IN STATICS.

PROF. H. R. HASSÉ, M.A., D.Sc.

IN spite of the great increase in the last fifteen years in the use of the Calculus in the higher forms of schools, and of its introduction into the Intermediate Examinations of many universities, it has not yet been used as much as it could be in the teaching of elementary Applied Mathematics.

It has, of course, long been realised that in the kinematics of the motion of a particle in a straight line we have some of the best examples of the ideas that are the basis of the Calculus, and the notation and machinery of the Calculus is now extensively used in dealing with this part of the subject.

What I propose to do in this article is to show how a more extended use of the Calculus in dynamics enables us at the same time to shorten and to improve on the proofs usually given of certain important results, and further to consider the bearing of these methods of proof on the principle of "virtual work" in statics, a principle which, unless the student is content to take it merely as a method of solving problems, presents many difficulties to him on its logical side.

Starting from the knowledge of the component of the acceleration along the tangent to the path described by a particle in two (and in three) dimensions, viz. $\frac{dv}{dt}$, where v is the speed of the particle, a result which is most simply deduced from the theory of the hodograph, the tangential equation of motion of the particle is

$$\frac{mdv}{dt} = F\cos\theta,$$

where F is the resultant force acting on the particle, and θ the angle that the direction of this force makes with the direction of motion.

It follows directly, on multiplication by v, that

$$\frac{d}{dt}(\tfrac{1}{2}mv^2) = Fv\cos\theta. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

This equation may be called the *activity equation*. It gives the time-rate of change of the kinetic energy on the left-hand side, and if the right-hand side, $Fv\cos\theta$, be defined as the rate of working of the force F, *i.e.* as the activity of the force, using the term given in Thomson and Tait, *Natural Philosophy*, Part I. p. 247 and p. 270, then the equation reads:

rate of change of kinetic energy = activity {rate of working} of force. .....(2)

Hence, on integration with respect to the time, we have, in any interval of time, and therefore in any displacement of the particle,

change of kinetic energy = work done by resultant force, ...(3)

which is the *energy equation* in its usual form.

I wish to point out the many advantages in using the activity equation (1) (or (2)) as a stepping-stone to the energy equation (3). Everything depends, of course, on the expression for the rate of working (or activity) of a force in the form $Fv\cos\theta$, and it is first of all necessary to show that forces may be combined together in calculating their activities. If $F_1, F_2, F_3 \ldots F_n$ are forces acting on a particle, and $\theta_1, \theta_2, \theta_3 \ldots \theta_n$ are the angles the directions of these forces make with the direction of motion of the particle, and F, θ refer to the resultant of these forces, it follows by resolving along the direction of motion that

$$F\cos\theta = F_1\cos\theta_1 + F_2\cos\theta_2 + \ldots + F_n\cos\theta_n$$

whence

$$Fv\cos\theta = F_1v\cos\theta_1 + F_2v\cos\theta_2 + \ldots + F_nv\cos\theta_n.$$

so that the activity of the resultant force is equal to the sum of the activities of the separate forces. On integrating this result, it follows that in any displacement the work done by the resultant force is equal to the sum of the works done by the separate forces in the same displacement. It should be noted that there is no restriction on the magnitude of the displacement, and that the proof applies to the case of forces variable in both direction and magnitude.

Since, then, the various forces acting on a particle can be treated separately, the activity of a force F_1 for which $\theta=\frac{1}{2}\pi$ always is zero at every instant of the motion, and therefore in any finite displacement the work done by this force is zero. Hence no work is done by the tension of the string in the motion of a simple (or spherical) pendulum, or by the reaction of a smooth curve or surface on which the particle is moving. Such a proof as this replaces the type in which the curved path of the particle is replaced by a large number of short straight portions, and an appeal is made to some limiting process which needs very careful consideration for its justification.

Again, consider the case of a force constant in magnitude and direction, such as gravity. If this acts along the negative direction of the axis of y, we have $Fv\cos\theta=-mg\,dy/dt$, and on integration the work done by the force is $mg\,(y_0-y)$, and thus equals the product of the weight of the particle and the decrease in level of the particle during the displacement. With these results, proved in a perfectly satisfactory manner, most of the problems in elementary dynamics, such as the motion of a particle in a vertical circle under gravity, can be discussed.

In order to show how to calculate the work done by a variable force, we write $Fv\cos\theta=T\,ds/dt$, where T is the tangential component of the force, and suppose a $T-s$ curve constructed. If A represents the "area under this curve," we have directly

$$\frac{dA}{dt}=\frac{dA}{ds}\cdot\frac{ds}{dt}=T\frac{ds}{dt}=\text{activity of force},$$

whence, on integration, A represents the work done by the force in any displacement. It is then necessary to consider under what circumstances the work done by the force in any motion of the particle from A to B depends on the path joining A and B, which leads to the definition of conservative forces, to the introduction of the term "potential energy," and to the proof that the sum of the potential and kinetic energies is constant. All this follows in the usual way, and therefore need not be given here.

The next step is to consider a system of particles. Since the activity equation (2) is a scalar equation, we can add up the equations for the separate particles of the system, and thus arrive at the result that the rate of change of the total kinetic energy of the system is equal to the sum of the activities of the forces acting on the particles of the system, and, on integration with respect to the time, to the result that

change of kinetic energy = work done by the forces.

The usefulness of this energy equation depends on the fact that in many cases the particles of the system are connected in such a way that the work done by certain of the forces acting on the system is zero. This is the case, for example, when two particles, connected by a string passing over a smooth peg, are moving in any manner in a plane, as in Fig. 1. The condition that the string remains taut is that the component velocities of the ends of the string along the string are the same. It follows directly that the sum of the activities of the two equal tensions at A and B is $(-T)\,.\,v+T\,.\,v=0$. This being true at each instant of the motion, it follows on integration that no work is done by the tension of the string in any displacement of the particles, small or large. The simplicity and rigour of this method of proof may be compared with that usually given (*e.g.* in Routh's *Statics*, vol. i. 2nd Edit.

1896) in which it is shown that in any *small* displacement of the system the work done by the tensions is of the second order of small quantities, leaving it to be proved that in the succession of such small displacemnts which constitute a finite displacement the total work done is of the first order of small quantities, the proof of which may in certain cases require careful treatment.

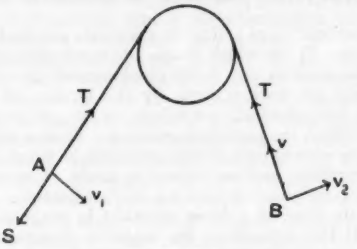


FIG. 1.

The omission of any consideration of this difficulty is more important when the number of particles composing the system is very large. If a rigid body is taken to consist of a very large number of particles connected together by very short light rods of invariable length, it is very necessary to prove that the work done by the forces in the rods does not, in a finite displacement of a body, amount to a finite quantity. By the methods of proof given in this article, the proof that no work is done by these forces follows directly from the fact that the activity of the pair of forces in each rod is zero at every instant, so that in any displacement no work is done by these "internal" forces, a rigid body being defined in the manner stated above.

From the above discussion, it is clear that it is just as important to show what forces will not contribute anything to the total work done as to calculate the work done by the remaining forces, and it is here that the activity equation is of great use in providing concise and rigorous proofs. The case of one body rolling on another is a further example. Since in pure rolling the velocity of the point of contact is zero, the activity of the reaction is zero, and hence no work is done by the reaction in any displacement,

Passing now to the principle of virtual work (in which the remark just made applies equally well), the difficulties inherent in the usual presentation of this principle are of exactly the same kind as those considered so far in this article. In addition, there is the question of the interpretation of the word "virtual" which, it is well known, has been given various meanings, and there is further the problem of deciding whether it would not be preferable to use the term "virtual velocities," as has sometimes been done. It would be interesting, if space permitted, to consider these questions in the light of the historical development of the principle as given in Duhem's *Les Origines de la Statique*. As, however, my object here is to present the principle in a form which is free from any ambiguity, I shall avoid the use of any terms about the meaning of which there is any doubt, and shall deal with it in the form of a *principle of zero activity*.

Consider any system of particles acted on by various external forces, and subject as regards their motion to certain geometrical conditions, *e.g.* that two particular particles are connected by a light string of invariable length, or that a particular particle must remain in contact with a given smooth surface. It has been shown that the tension of the string and the reaction of the surface do not contribute anything to the activity of the system of forces at any instant provided these geometrical conditions are satisfied. Such a motion

is called a constrained motion, and the "constraints" are termed "frictionless" if they are of the above type in which the activity of the constraining forces is zero at every instant. We may, however, agree to abolish the string and surface, and to look upon these as replaced by the tension and the reaction respectively. The system of particles is then free to move in any manner under the combined action of the external forces and the forces by which the constraints have been replaced.

The principle of zero activity deals with the activity of the forces during the motion of such a system of particles, and asserts that if the activity of the forces at any instant is zero for all possible motions, then the position of the system at that instant is one of statical equilibrium, and conversely.

A general proof can be given for a system having one degree of freedom. This is based on that given in Thomson and Tait, *Natural Philosophy*, Part I. p. 266, and is as follows. Consider the system in motion, each particle moving along a particular path which is definite as soon as the motion of a single particle is given. At any instant the activity of the forces acting on the particles is $\Sigma(Fv)$, where F is now the component of the resultant force on any particle along its direction of motion, and v is the speed of the particle in its path.

(1) If the particular position is a possible position of equilibrium, the force F must be zero, since this is the sole condition of equilibrium for each particle, and therefore the activity of the forces is zero.

(2) Suppose that $\Sigma(Fv)$ is zero for a particular position of the system. If this position is not one of equilibrium, it can be clearly made so by introducing a force on any *one* particle equal and opposite to that actually acting on it. If F_1 is the component of this force in the direction of the velocity v_1 of the particle chosen, then, since the system is passing through a possible position of equilibrium, we must have by (1) above,

$$\Sigma(Fv) - F_1 v_1 = 0.$$

But $\Sigma(Fv)=0$ by hypothesis, and therefore $F_1=0$, so that no force is necessary in order to make the position one of equilibrium, *i.e.* the system is passing through a possible position of statical equilibrium.

In the application of this method to statics, we meet with two types of problems, one in which we require to know what are the possible positions of equilibrium under the action of given constraints, and the other in which we require the value of a constraining force in order that a particular position may be one of equilibrium. In the first type of problem we want to obtain an equation free from the unknown constraining forces, so that in applying the principle of zero activity we naturally consider only those possible motions of the system in which these forces do no work, *i.e.* motions in which the geometrical conditions of the problem are not violated. In the second type of problem we look upon the reactions due to the constraints as additional forces acting upon the system, and consider a motion in which only the particular constraining force whose value is required will appear in the expression for the activity. Since the position chosen is one of equilibrium, the activity of all the forces is zero, and this will determine the value of the particular reaction.

The usefulness of the principle of zero activity in the solution of statical problems lies in the fact that it enables us to concentrate on one thing at a time, either the determination of possible positions of equilibrium or of a particular constraining force. It is therefore evident that the limitation of the above proof of the principle to cases of one degree of freedom is not a serious disadvantage, as we set out to obtain only one equation, and therefore consider only one of many possible motions of the system. It is, however, easy to extend this proof to a system with any number of degrees of freedom by modifying that given by Thomson and Tait referred to above.

Take as an example of the second type of problem the old one of a hexagon of jointed rods hung up at one corner A and kept in the regular form by means of two horizontal light rods BF and CE.

In order to find the stresses in these rods, we replace the constraints by the forces T, T' acting as in Fig. 2. In order to find the values of these forces

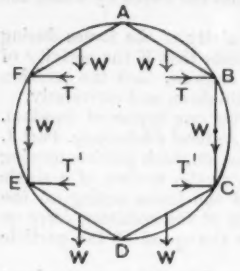


FIG. 2.

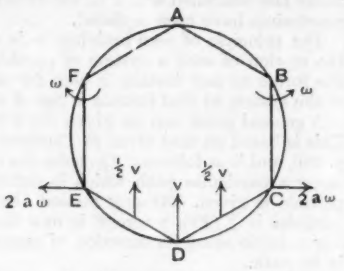


FIG. 3.

in turn, consider a motion of the system through the position of equilibrium in which ABF remains fixed and D moves vertically. The velocities of the various points of the system are given in Fig. 3, whence it is clear that the vertical components of the mid-points of CD and DE are each equal to $\frac{1}{2}v$, and that the connection between v and ω is given by the fact that the velocities of D and C along the rod DC must be the same, so that

$$v\cos 60° = 2a\omega\cos 30°, \quad \text{or} \quad v = 2a\omega\sqrt{3}.$$

From Figs. 2 and 3 the activity equation gives

$$W(-\tfrac{1}{2}v) + W(-\tfrac{1}{2}v) + T'.2a\omega + T'.2a\omega = 0$$

whence $$T'4a\omega = Wv, \quad \text{or} \quad T' = (\sqrt{3}/2)W.$$

Again, if we take a motion of the system in which ECD retains its shape, and D moves vertically, the velocities are given in Fig. 4, in which the con-

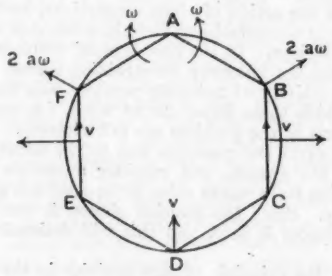


FIG. 4.

nection between v and ω is given by making the vertical component of the velocity of B equal to the velocity of C, so that $2a\omega\cos 30° = v$. Applying the principle of zero activity, we have

$$-2Wv - 2Wv - 2Wa\omega\cos 30° + 2T.2a\omega\cos 60° = 0,$$

whence $$-10a\omega\cos 30°.W + 4Ta\omega\cos 60° = 0,$$

or $$T = (5\sqrt{3}/2)W.$$

The advantage of this method of proof over that in which small displacements are considered is again one of precise statement; there is no need to draw a figure of the system in a displaced position, and to investigate the work of all the forces, omitting those forces which contribute terms of the second and higher orders to the work done in the small displacement of the system.

In order to use this method, the velocities of all points of the system must be known; the necessary knowledge, in problems in two dimensions, can often be easily obtained from the theory of the instantaneous centre. As an example, the equilibrium position of a rhombus of jointed rods in which AB, AD rest on two smooth pegs at E and F in the same horizontal line, as in Fig. 5, can be determined as follows. Replace the pegs by equal forces R

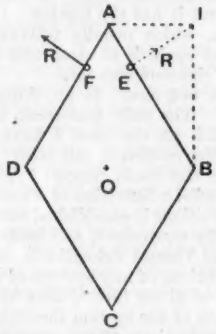


FIG. 5.

perpendicular to AD and AB, and consider a motion of the system in which A and C move vertically, B and D horizontally, so that the position of the centre of gravity remains unchanged at O. The activity of the weights of the rods is therefore zero, so that that of the forces R must be zero also. Hence the direction of R must pass through the instantaneous centre I for the motion of the rod AB, so that the peg F must be placed at the foot of the perpendicular from I on AB. The condition $a\sin^3\theta = c$, where $2c$ is the distance between the pegs, a the length of any rod, and θ the angle that a rod makes with the vertical, to which the above statement is equivalent, is more usually obtained by considering a motion of the system in which the upper rods remain in contact with the pegs, so that the forces R do not appear in the activity equation, and it is then an example of the first type of statical problem.

In order to complete the statement of the principle of zero activity, it is necessary to consider the case of a single rigid body. Limiting the consideration to two dimensions, let (X, Y) be a typical *external* force (it has already been proved that the *internal* forces do not appear in the activity equation) acting at the point of the body whose component velocities are $(u - y\omega, v + x\omega)$, where (u, v) are the component velocities of any particular point G of the body, and (x, y) are the coordinates of the point where the force is acting referred to rectangular axes of coordinates through G, and ω is the angular velocity of the body. The activity of the force (X, Y) is

$$X(u - y\omega) + Y(x + v\omega) = Xu + Yv + \omega(Yx - Xy).$$

Summing for all such external forces, their total activity is

$$u\Sigma(X) + v\Sigma(Y) + \omega\Sigma(Yx - Xy),$$

which shows that it is equal to the activity of the force and couple at G to which the external forces are statically equivalent.

It follows directly (i) that, if the body is moving through a position of equilibrium so that $\Sigma(X)=\Sigma(Y)=\Sigma(Yx-Xy)=0$, then the activity of the external forces is zero, and (ii) that, if the activity is to be zero for *all* motions of the body through the given position, then $\Sigma(X)$, $\Sigma(Y)$ and $\Sigma(Yx-Xy)$ must each be zero, so that the position is a possible position of equilibrium. This proves the principle of zero activity for a single rigid body, and is on the lines of that given in the standard text-books. It is, of course, possible to put the proof into a more geometrical form, if considered desirable.

If this method of dealing with statical problems of the type considered above is adopted, it will of course have to be included in the dynamical rather than in the statical portion of the course in Applied Mathematics. The former is, I think, its proper place, and it has the further advantage that the question of stability of equilibrium, which usually follows, is then treated from a dynamical point of view, as again is natural, and indeed essential if it is to be done in any careful and rigorous manner.

Of course this method is not new. Is anything that is published in the *Mathematical Gazette* new? The only text-book, however, in which I have found the subject dealt with on the lines I have indicated is Todhunter's *Analytical Statics*. In the fifth edition of this book (1887), which was edited by Everett, the last chapter in the book, headed "Work, and the Principle of Virtual Velocities," begins with a definition of $Fv\cos\theta$ as the "work-rate" of a force and a series of propositions is established concerning the "work-rates" of systems of forces statically equivalent, and leads up to a general statement and proof of the Principle of Virtual Velocities in the form: "It is necessary and sufficient for the equilibrium of any system of bodies, in a given position and subject to the action of given forces, that the work-rate of the forces shall vanish for every motion of the system through the given position."

Though the very few applications of the method which are given do not show its advantages over the method depending on small displacements, and although the idea of the work-rate (activity) of a force as being the fundamental idea is not used to the extent that is possible, yet this chapter contains the essentials of the latter half of this article. It is interesting to note, firstly, its position as the last chapter of the book—which is well on the way to its displacement from Statics altogether, and to its inclusion in Dynamics—and, secondly, that, according to the preface, almost the whole of the chapter is new, so that it is probably due to Everett himself. I have not seen a copy of the fourth edition of the book so I cannot say what are exactly the additions made by Everett.*

I should like to add that this article owes its origin to a discussion which took place at a meeting of the Bristol Branch of the Mathematical Association in February at which Mr. G. W. Hinton, B.Sc., of Cotham Secondary School, read a paper on "The Principle of Virtual Velocities," in which the historical development of the principle was treated.

H. R. Hassé.

---

475. The original MSS. of Roger North's *Life* says: he was a Johnian.... And made great Advances in the mathematiques, and being very capable had in due time fulfilled an utmost cours therein, but in that his affairs of more Importance sayd, Nay. But yet he was alwais a good judge of New propositions, in the Synthetick way (the Analitick was not so much in use in his time as since), and if they faild, shewed where. He was admitted June 8th, 1653.—v. *N. & Q.* X. ix. 201.

---

*I am indebted to the Editor for sending me a copy of the fourth edition, in which the subject is treated on the basis of the idea of small displacements. Hence the new method of treatment in the fifth edition is due to Everett.

THE LAWS OF MOTION.

A DIALOGUE.

By G. H. Lester, M.A.

"The time has come," the Master said,
"To talk of many things,
Of trains and wheels and little sticks
And tensions in thin strings,
Of lunar tricks and lunatics
And why a kettle sings."

Scene: A prep. room at any school.
Time: 7.45 p.m.
Characters:
John, a sixth-former of average intelligence and industry.
The Shade of Newton.

John (*with a sigh of relief*). Well, that does old Spectroscope's physics anyhow. Now I suppose I've got to start that beastly essay on the laws of motion. Though why the ass wants it I cannot think! What's the good of him saying I don't understand them when I can do the problems on them as often as not? Besides, whatever I say he'll try to pick holes in it—seems to think he knows more about mechanics than anyone else, even the people who write books about it. I believe he would argue with Newton himself if he got the chance. I wish the old bean was here—we'd have some fun.

(*The Shade of Newton rises suddenly through the floor with uniform velocity in a straight line. He is somewhat untidily dressed—even the extra two hundred years have not taught him to fasten his doublet on the proper buttons.*)

Newton. Good evening, my friend. Pray don't look so surprised. I am always ready to give help to those who seek it, but it is only rarely that any of you think of calling on me.

John. Oh! Good evening. Please don't talk quite so loud or Nosey will hear you. How did you get in?

Newton. Quite simple. Interpenetrability of matter and that sort of thing doesn't apply to spirits, you know—at least not to my kind of spirits. That's why I was able to come to rest just now without the application of an external force. Now, what's this little difficulty of yours about the Laws of Motion?

John. Well, the fact is that the chap who's supposed to teach us maths has set us an essay on your laws. He gave us two preps to do it in, but I had to spend the first one in swotting for a Cert. A exam and I've got to give in my essay to-morrow. Of course Cosine jawed a lot about it in school yesterday, but to tell you the truth I didn't understand much of what he said, and I'm in an awful funk—just in case he says it all over again, you know.

Newton. Yes, yes. What a dreadful prospect. We must avoid that at all costs. Let us write down the laws one by one, and see whether we can get our ideas clear.

John (*sniggering*). Just what Cosine said. Well, here goes. (*He writes.*) Law I.—Every body continues in its state of rest or uniform motion in a straight line, unless made to depart from it by the action of a force.

Newton. Ah, so you are familiar with the enunciation of the laws?

John. Rather. I've known them donkey's years, but Cosine never seems satisfied with facts—he always wants patter as well.

Newton. Well, I'm afraid we can't do much in that line over the first law. What's more, I really must disclaim the credit for it; it was quite a current idea before my time, and I only put it down to clear the ground. Poor old Galileo knew all about it—used to call it the Law of Inertia. That was before he was run in for saying that the earth moves round the sun.

John. Coo! Did that really happen? I thought it was just one of Cosine's yarns.

NEWTON. Oh, yes, as a matter of fact the poor chap died for his conviction—forgive the pun. That was in 1642, the very year I was born. By the way, it's an interesting fact that we've never been able to verify the Law of Inertia. You see while we were still on earth we could never arrange an experiment with a body acted on by no forces.—We couldn't get rid of the weight. Your novelist Wells, before he took to politics, wrote an amusing story about a man who discovered a substance that was impervious to gravitation, but apparently didn't use it, as I should, for a sensible scientific experiment—he went off to the moon instead. Of course now that we're on the astral plane we're just as badly off as before, because nothing has any mass, we can't do any experiments because a force can't exist unless there is a body for it to be exerted on.

JOHN. Rotten luck; you must miss your practical periods. But surely there are experiments to prove the First Law? We've got a thing called "Arrowsmith's Trolley" . . .

NEWTON (*interrupting*). For the love of Archimedes, don't mention that contraption to me. I never see that it proves anything—but it will never do for the Mint to argue with the Board of Education about teaching.

JOHN. Sorry, Sir. As a matter of fact, I never did really understand that experiment.

NEWTON. Well, forget it. If you want a sensible remark about the First Law, I should just say that it's better not to regard it as a law at all but as a definition of force. It comes to the same thing as saying that "Force is that which produces or tends to produce motion of matter." Now, next please.

JOHN (*writing*). Law 2. Rate of change of momentum is proportional to the force and takes place in the direction of the force.

NEWTON. Now, let us define our terms, as the philosophers say. We might as well start with momentum. If you'll excuse symbols, that simply means mv when m is the mass and v . . .

JOHN (*interrupting*). Yes, excuse me, but what exactly do you mean by mass? Cosine says it's easier to see what it means than to define it. Surely you can explain the point if any one can!

NEWTON. You flatter me. I sympathise deeply with your worthy friend, for I failed dismally myself to give a sound definition—of course my statement that mass is a quantity of matter in a body means nothing in particular, but I had to say something. Later scientists have improved the position, but their explanation would not appeal to you just yet. In a year's time you might ask Cosine about it, or read it up in Roberts' *Dynamics* or the *Encyclopedia Britannica*. For the present you must be content with a more or less instinctive view of mass. You know that the weight of a body, as measured with a spring balance, is different in different parts of the earth, and would decrease as much as you like if you were to go far enough away from the earth. But the mass, as measured in terms of standard masses with an ordinary balance, is the same everywhere—at the Equator or at the Poles, on the earth or at the moon. I must ask you to accept this partial explanation, and allow me to get on with the washing, as dear Leibniz used to say, or rather with the clearing up.

JOHN (*condescendingly*). Well, it's curious that you don't seem to know any more about it than Cosine, but I must say I have an idea what you're getting at. Please go on.

NEWTON. Let us continue symbolically. The law gives $P=kmf$; k would be a constant nuisance, and we avoid it by taking the unit of force to be that which produces unit acceleration in unit mass, so that $P=mf$. In the F.P.S. system the unit of force is called the poundal, an unfortunate and confusing name. (*Slyly.*) Perhaps you have found it so?

JOHN. No, I think Cosine made that fairly clear. He explained how Galileo experimented on falling bodies—spent a half holiday dropping things off the leaning tower of Pisa, you know.

NEWTON. Such a thorough investigator! And he had a really dreadful head for heights! But I interrupt you.

JOHN. That's nothing, you should hear Cosine. Well, Galileo made certain that at any particular place all bodies fall with the same acceleration say g ft./sec.^{2}

This acceleration must be caused by the force called the weight of the body, and your second law gives us $W=mg$; in other words, a body whose mass is m pounds has a weight of mg poundals, and this is sometimes called m pounds-weight.

NEWTON. A very lucid exposition. You are lucky in your teacher—mathematics was not made so easy in my young days. Now, if you will allow me, I should like to give you one or two hints for avoiding the grosser forms of howling in using this law. The most important point to notice is that you must fix your attention on a particular body, and consider only forces which act on that body. Further, you must be careful to include all the forces which act on the body; that is, to remember that the P of my equation is the resultant force on the body in the direction of the acceleration f. Only yesterday I was looking over the shoulder of a youth who was making this stupid mistake. He was working a question about a man tumbling into a river, and calculated his retardation from the formula $T=mr$ instead of $T-mg=mr$, T being the upward thrust of the water. The funny fellow was surprised to get such a large answer. If Nature really worked like that, they would never have managed to drown Clarence in that butt of Malmsey, and my little Diamond's puppies would have filled half Cambridge.

JOHN (*his conscience not quite clear*). But surely that is a question of hydrostatics? or even hydrodynamics?

NEWTON. As you please, but after all my laws are the foundation of all mechanical science. For example, the laws of statics are simply the laws of motion with $f=0$ in Law 2. In fact, the second law of motion, besides providing us with a means of measuring force, solves all possible problems on the motion of a single body. When, however, we come to deal with the mechanics of two or more bodies we must add a third law of motion . . .

JOHN (*writing*). Action and reaction are equal and opposite.

NEWTON (*scathingly*). I feared as much—Hooke himself was not more obscure. Take my advice, my boy, and shun snappy and symmetrical sentences which serve only to conceal an abysmal ignorance of the scientific statements whose place they usurp. Always say exactly what you mean, even if there is some loss of elegance in the result. Allow me to dictate. (*John takes it down.*) "If a body A exerts a certain force on a body B, then B will simultaneously exert an equal and opposite force on A."

JOHN (*somewhat dashed, but feeling that he is now on safer ground*). Yes, I suppose that does make it a bit plainer. Now I hope you won't be offended with me; nobody thinks more highly of your first and second laws than I do, but I must say that the third seems to me rather obvious, and I have heard other people say the same. (*He looks enquiringly at Newton, who at first seems angry, but gradually melts into a hearty laugh.*)

NEWTON. Well, upon my word, that beats the band. When you reach the age of two hundred and fifty or so you can generally make some sort of an estimate of the value of your life's work. Now I did something in most branches of physical science, and not least in dynamics, but I do not hesitate to say that my greatest contribution to human thought was this law of reaction. And you tell me it is obvious.

JOHN (*bewildered*). I can see that there may be something in what you say as far as dynamics is concerned, but surely in statics the thing is plain enough. Take for instance, as Cosine always does, the case of the book lying on the table. Since the book does not move its weight must be equal and opposite to the upward pressure of the table.

NEWTON (*icily*). Quite so, go on.

JOHN. Well there you are.

NEWTON (*pulling himself together by a great effort*). My friend, let us see exactly how we stand. Have you been reading, by any chance, a book by a person called Tinker ?

JOHN. Of course—we all do.

NEWTON. That accounts for everything. I shall not express my sentiments on this unpleasant subject—spirits are notoriously inflammable. Let us try instead to correct your wrong impressions. The equality of the forces in your example is an instance of the first law of motion, not of the third, both your forces act on the *book*, and what you are saying is that as the book does not move the resultant force on it must be zero. A pair of forces which really are action and reaction are the pressure of the book on the table and the pressure of the table on the book; my third law states that these forces are equal and opposite, though I see no intuitive reason for supposing so. Neither did Leibniz nor Hooke nor any other scientist of my time think it obvious. Of course they did not have the advantage of learning their mechanics from Tinker.

JOHN (*shifting his ground*). But if the two pressures you mention are not equal, the book will move.

NEWTON. Oh, generation of vipers ! The pressure of the book on the table is a force acting on the table, how can it possibly affect the motion of the *book* ?

JOHN (*with a last wriggle*). Yes, I see now that the equilibrium of the book has nothing to do with the question; perhaps there is more in your third law than I had supposed. But surely it can be proved in simple cases—for example, by hooking two spring balances together and noting that their readings are the same.

NEWTON. A common fallacy, and a most pernicious one. The argument is quite inadmissible, for every time you measure a force with a spring balance you assume the third law of motion. Indeed, but for this law the mere expression " tension of a string or spring at the point P " would be meaningless. I have here a piece of astral string as supplied to the Recording Angel for tying up documents. The rhyme of the Cambridge professor :

> " Hence no force, however great,
> Can stretch a string, however fine,
> Into a horizontal line
> Which shall be absolutely straight,"

does not apply to this string as it is weightless, and I therefore stretch it out horizontally. Now when I say " the tension of the string at P," do I mean the force with which the right-hand section pulls the left-hand section or *vice versa* ?

The answer, but for my third law which states that the two forces are actually equal, would be the proverbial but ambiguous lemon.

JOHN. I give in. But if it is not possible to prove these laws of yours by experiment, why should we suppose that they are true ?

NEWTON. A very sensible and interesting question. In the history of every scientific law there are four distinct stages. First, we observe the phenomena in which we are interested. Secondly, we suggest some law which fits the facts as far as they are known to us, and, thirdly, we try to discover, by mathematical or other processes, what are the logical consequences of the suggested law. Fourthly, by fresh observation and experiment we test the truth of these consequences, and if our results are in agreement with our predictions, we regard the law as established. No better example of this " method of science " can be found than the discovery of the laws of motion. The first stage of observation and experiment was chiefly due to Tycho

Brahe, Galileo and Kepler. The second stage was my suggestion of the laws of motion. The third stage was a long business, but I confess that I enjoyed it. By means of the laws of motion, and that of universal gravitation which I had just put forward, I explained the phenomena of the tides, of the obliquity of the earth, and of the precession of the equinoxes. I was also able to explain various irregularities which had been observed in the orbit of the moon, such as the evection, the variation, the annual equation and the regression of the nodes. In working out these results I found that if my laws were true there must be two other small irregularities, the inequalities of the apogee and of the nodes, which had never been noticed at all; soon afterwards their existence was verified by astronomers, and my position was enormously strengthened. Many years later an even more remarkable thing happened. Astronomers were perplexed by some peculiar disturbances in the orbit of the planet Uranus, and two scientists, Adams and Leverrier, found that if there were another and larger planet moving in a very large orbit in a particular way, which they were able to describe from their calculations, its attraction on Uranus would account for the disturbance. They sent their results to the astronomers, who turned their telescopes in the suggested direction, and saw that the new planet, never noticed before, was really there. It is on dramatic verifications of this kind that my belief in the laws of motion is based. But, of course, if Tinker is able to see their truth off-hand, I can only take off my hat to a superior intellect! However in a year or two I shall be Chairman of the Examiners when he takes his matriculation at the Astral University, and I shall have some interesting questions to ask him.

JOHN. Well, I hope you will get more out of him than I have done. Now don't think me cheeky if I ask you rather an awkward question. Is it a fact that this man Einstein has gone and upset your laws altogether? And what is this relativity business anyhow?

NEWTON. I can hardly start talking relativity at this time of night—and I don't think I really want to. It seems to me that relativity is a purely mathematico-physical theory which requires for its proper exposition a considerable knowledge of differential algebra and modern electromagnetic theory. I am no believer in the search for "relativity without tears," and, what is more, I have no particular use for the ink-slinging with which the non-scientific philosophers have greeted the work of Einstein and his predecessors.

There are, however, two points which I think I can fairly put before you. Firstly, Einstein's theory, as applied to mechanics, is only a modification of mine. The results of the two theories are identical, except for phenomena where the velocities dealt with are comparable with that of light. Even there the differences are minute, so there is no occasion to think that you are wasting time in studying the classical system of mechanics. Secondly, I should like to show you how Einstein's work has followed the "Method of science" which I tried to explain to you. As with Adams and Leverrier, his original problem was to explain a discrepancy which had already been observed and did not seem consistent with my laws. He evolved his theory to fit this known fact, then he set to work to find out and test the consequences of it. This was difficult, because it is only rarely in celestial and electrical phenomena, and never in ordinary mechanical experiments, that we meet the high velocities necessary to make the relativity correction perceptible. However, he was able to make two predictions, one about the deflection of starlight passing near the sun, and one about a shift in the lines of the spectrum of a distant star. The first prediction was verified at the 1918 solar eclipse—it can be tested only at an eclipse because normally the starlight is drowned by the sunshine. His second prediction is not yet proved or disproved because the prophesied shift in the spectrum lines is mixed up with another shift caused by pressure, and it is not yet possible to separate the effects so as to measure either with accuracy.

So you see that Einstein, as far as his own work is concerned, is two up and one to play. But another German physicist, Sommerfeld, has strengthened the case for relativity by applying it to modify the quantum theory of the hydrogen spectrum and successfully using it to account for the "fine structure" of the spectrum. Thus, at the moment, the evidence is very definitely in favour of relativity.

JOHN. Well, it will be interesting to see which of you is right.

NEWTON. We are both right. Both of us have done all that any scientist can do; we have explained the facts as we knew them and we have sought to test our explanations—angels could do no more. Nothing that Einstein has done can lessen the value of my own small labours, and nothing any later scientist may discover—even though it lead to the complete rejection of relativity—can dim the glory of Einstein's great achievement. But let me apologize for my donnishness—for the last ten minutes I have quite thought myself back in the good old Lucasian Chair at Cambridge.

A VOICE. Now, boys, finish your prep.

NEWTON. I must be off at once. Good luck. (*He disappears with perceptible acceleration.*)

JOHN. I must have been asleep. Eight-thirty, and I've only written down the laws of motion! And what is this tosh I've got instead of Law 3? (*Crosses out Newton's production and writes instead* "*Action and reaction are equal and opposite.*") Cosine will be mad, but I can copy out some bits of Tinker to-morrow before school.

G. H. LESTER.

---

476. *The Principles of Natural Philosophy*, by R. Greene, Fellow of Clare, 1712. The English translations of Algarotti's work on *Newton's Theory of Light and Colour* (1742, 2 vols.) has a story (footnote 1, p. 132) which seems to have escaped De Morgan in his remarks on Greene.—Cf. *Budget*, i. p. 135 (1925). The celebrated Mr. Cotes, Professor of Astronomy, used to say, that this Book showed the Author to have had as extraordinary a Genius as Sir Isaac Newton's; since it must have been the Effect of Design to guard so effectually, as he did, against saying any one right Thing in so large a Treatise.

477. It is by the predominant influence of this rare quality (the splendour of original genius) that the sermons of Horsley are freed from that dry severity of ratiocination which never fails to cramp the style of ordinary mathematicians, when writing on theological subjects. Proficiency in this science, we mean as distinct from invention and discovery, is no decisive test of superior talents. Great perseverance united to ordinary understandings will suffice for the purpose: but great perseverance long employed in this single direction will, if such qualifications have been bestowed on the mathematical student in a moderate degree alone, extinguish imagination, check the flow of native eloquence, and cramp every movement of free and excursive rhetoric. It is only minds like those of Barrow and Horsley (for the influence of the mathematical curb is visible in the discourses of Clark) in which the vigour of the more elastic and animate faculties is not broken by pertinacious meditation on abstract science; for where the acquirement is difficult, the whole understanding is absorbed; the mind takes a single ply, and when bent by a long and laborious attention to mathematical truths, becomes so tense and rigid, that it never after applies itself to moral or religious truths with ease and grace. In intellects of an order so superior, the other faculties, vigorous from the beginning, remain unimpaired, while the student passes at will and with ease from the exercise of pure analysis, to the ever varying gradations of moral evidence, to the persuasive topics of rhetoric, or to the awakening and animating strains of popular eloquence. A union of excellencies so rare, and, in general, so little compatible, we repeat it, has hardly been attained but by Barrow and Horsley among the divines of this or any other age or country.

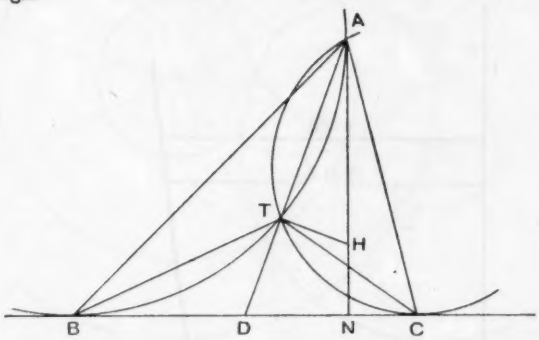
MATHEMATICAL NOTES.

882. [K¹. 2. d.] *A Note on the Geometry of the Triangle.*

In making a selection for junior specialists from the mass of material included under the head of Geometry of the Triangle it is often convenient to choose items which lend themselves to the drawing of an accurate figure of the fairly elaborate kind which delights the large class of boys who revel in the use of coloured inks.

Among such figures one of the most attractive is that to be described. On each side of $\triangle ABC$ as chord, construct on the same side as the triangle three circular arcs to contain angles $180° - A$, $180° - B$, $180° - C$ respectively. There will in general be six points in which the nine circular arcs intersect by threes.* Calling the arc on BC to contain an angle $180° - A$, the arc a_1, and using a similar notation for the others, the point $a_1b_2c_3$ is the orthocentre and the points $a_2b_3c_1$ and $a_3b_1c_2$ are the Brocard points.

The remaining three points $a_1b_3c_2$, $a_3b_2c_1$, $a_2b_1c_3$ have a variety of interesting properties, and seem to have been neglected in accounts of the Geometry of the Triangle.



The figure shows the first of these points ($a_1b_3c_2$) found as the intersection of the arcs b_3, c_2 which are the arcs through A touching BC at B and C respectively.

If AT meets BC at D, $BD^2 = DT \cdot DA = DC^2$; $\therefore$ T lies on the median.

The angles BTD, CTD are B and C. Hence

$$BT : TC = \frac{BD \sin BDT}{\sin B} : \frac{CD \sin CDT}{\sin C} = \sin C : \sin B = BA : AC,$$

so that T also lies on the Apollonius circle which this relation determines.

Again $$DT = BD^2/DA;$$

$$\therefore\ TA = (DA^2 - BD^2)/DA = (4DA^2 - BC^2)/4DA = (b^2 + c^2 - a^2)/2DA;$$

$\therefore$ $AT \cdot AD = bc \cos A = AH \cdot AN$ when AN is the altitude and H the orthocentre.

Hence it follows that HT is perpendicular to the median.

Thus the three points, other than the orthocentre and the Brocard points, determined by the intersection in threes of the nine circular arcs, are the feet of the perpendiculars from the orthocentre to the medians. C. O. TUCKEY.

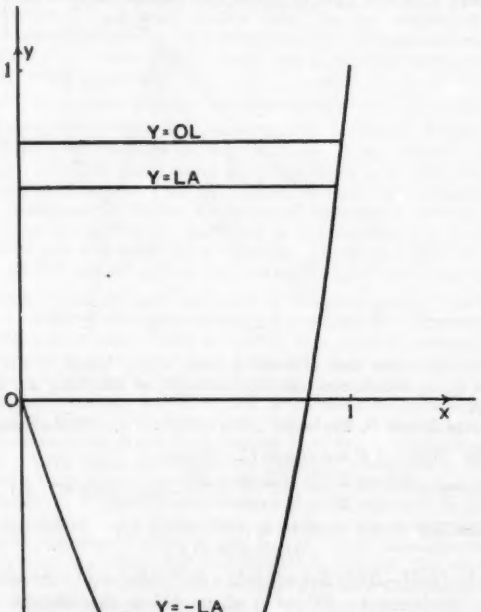
* The triangle should not be drawn isosceles, in which case the number of arcs is reduced to six and the points to three. If the triangle has an angle of 60° there is a seventh point where three of the arcs meet, viz. that point at which each of the sides subtends 120°.

883. [K¹. 21. b.] *The cubic $y=4x^3-3x$ as a means of trisecting any angle.*

Since $4\cos^3\alpha-3\cos\alpha=\cos 3\alpha$, the determination of $\cos\alpha$ when $\cos 3\alpha$ is given is equivalent to the determination of x when $4x^3-3x$ is given, with the restriction that x is to be numerically not greater than unity. If then we plot the curve $y=4x^3-3x$ for values of x from -1 to $+1$, and cut this curve by the line $y=\cos 3\alpha$, the values of x at the points of intersection will be the possible values of $\cos\alpha$. Since the curve is symmetrical about the origin, it will be sufficient if we draw the curve only for values of x from 0 to 1, and cut this curve by the two lines $y=+\cos 3\alpha$ and $y=-\cos 3\alpha$; if x is an abscissa of an intersection of this part of the curve with the second line, $-x$ would have been the abscissa of an intersection of the more extended curve with the first line.

Moreover, since $4\sin^3\alpha-3\sin\alpha=-\sin 3\alpha$, the intersections of the same curve with the lines $y=-\sin 3\alpha$ and $y=+\sin 3\alpha$ will give us the values of $\sin\alpha$, and the association of a value of $\sin\alpha$ with a value of $\cos\alpha$ will enable us to identify α geometrically without ambiguity.

This is the basis of the accompanying figures. The first gives the relevant part of the cubic curve, the second shows its use with an acute angle AOB,

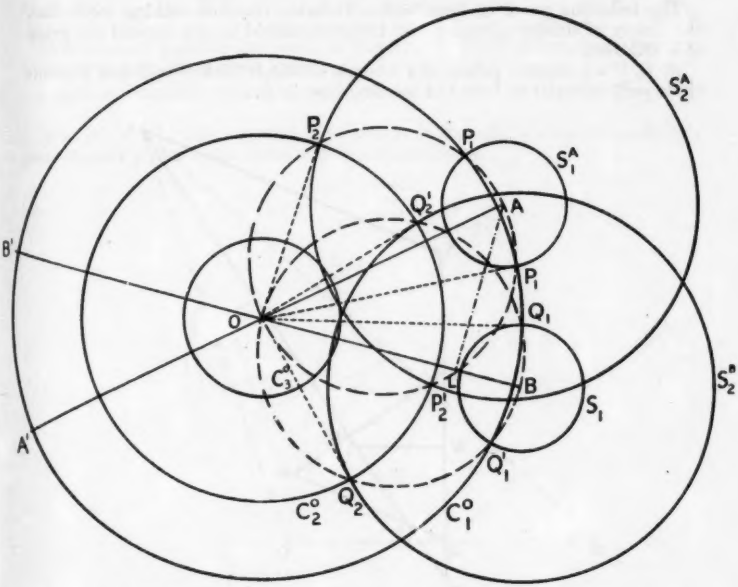


and also with the supplement of AOB: the extension hardly complicates the construction, since the same two pairs of lines $y=\pm\cos 3\alpha$, $y=\pm\sin 3\alpha$ serve

for the supplement of 3α as for 3α itself. Except for the drawing of the cubic, the construction is Euclidean.

Mark off along the arms of the angle lengths OA, OB equal to the unit length used for the cubic, and draw AL perpendicular to OB.

Cut the cubic by the line $y=OL$. The intersection gives the value, say c_1, of cos α, and we describe a circle $C_1^{\,o}$ with c_1 for radius and O for centre.



Similarly, the intersection of the cubic with $y=-LA$ gives two values s_1, s_2, of which the smaller, say s_1, is sin α, and the larger is the sine of one-third of an angle with the same sine as 3α, that is, is the sine of one-third of the supplement of AOB. We describe circles $S_1^{\,A}$, $S_2^{\,A}$ with radii s_1, s_2 and centre A, and circles $S_1^{\,B}$, $S_2^{\,B}$ with the same radii and centre B.

The intersection $C_1^{\,o}S_1^{\,A}$ gives two points P_1, P_1' of which the one marked P_1 is on the trisector of AOB, that is, nearer to OA; similarly, $C_1^{\,o}$, $S_1^{\,B}$ gives a point Q_1 on the trisector nearer to OB.

The intersections $C_1^{\,o}S_2^{\,A}$ and $C_1^{\,o}S_2^{\,B}$ are not orthogonal, which is to say that $c_1^{\,2}+s_2^{\,2}\neq 1$; there is nothing to associate these intersections with trisectors of any angle in which we are interested. But, as we have seen, s_2 is the sine of $\frac{1}{3}AOB'$, where OB' is the radius opposite to OB. To find the cosine of this angle we cut the cubic by $y=-OL$, obtaining two values c_2, c_3 of x which are the radii of the two circles $C_2^{\,o}$, $C_3^{\,o}$. One of these circles is orthogonal to $S_2^{\,A}$, and this is the one to be denoted by $C_2^{\,o}$; of the intersections $C_2^{\,o}$, $S_2^{\,A}$, the one marked P_2 is on a trisector of AOB', while Q_2, one of the intersections $C_2^{\,o}S_2^{\,B}$, is on a trisector of $A'OB$.

As for the circle $C_3^{\,o}$, this assists us to trisect an angle which has the same cosine as AOB' but a different sine; the sine of this angle is therefore $-LA$, and to find the sine of the corresponding one-third we have to cut the cubic

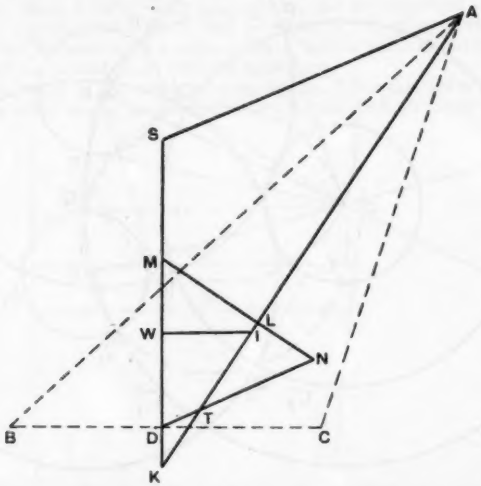
by $y=LA$, obtaining the radius s_3 for circles S_3^A. S_3^B, not drawn in the figure, which would intersect C_3^O on trisectors of the reflex angles AOB', $A'OB$.

If the circles on OA, OB as diameters are drawn, the intersections of pairs of orthogonal circles can easily be discriminated and irrelevant intersections ignored. W. J. LANGFORD.

884. [K¹. 2. d.] *Feuerbach's Theorem.*

The following proof of Feuerbach's Theorem requires nothing more than the theory of similar triangles, and therefore should be not beyond the grasp of a fifth form :

A, B, C are angular points of a triangle of which SDK is radius of circumcircle perpendicular to base and meeting base in D.



By a well-known theorem I lies on KA and $KI=KB$ or $KC=\sqrt{2Rd}$, where R is circumradius and $d=DK$.

Draw DN parallel to SA and equal to $\frac{R}{2}$, then N is easily seen to be the nine-points centre. Let DN cut KL in T.

Draw NLM perpendicular to KA meeting KA in L and KDS in M, and finally draw IW perpendicular to KS.

Proof. KA and its perpendicular NLM are equally inclined to directions SA and SK ;

$$\therefore\ DM=DN=\frac{R}{2}\quad\text{and}\quad DT=DK=d\ ;$$

$$\therefore\ MK=\frac{R}{2}+d\quad\text{and}\quad NT=\frac{R}{2}-d\ ;$$

$$\therefore\ \text{by similar triangles,}\quad \frac{ML}{KM}=\frac{LN}{NT}=\frac{IW}{KI}\ ;$$

$$\therefore\ \frac{ML^2-LN^2}{KM^2-NT^2}=\frac{IW^2}{KI^2},$$

$$\text{or}\quad \frac{ML^2-LN^2}{\left(\frac{R}{2}+d\right)^2-\left(\frac{R}{2}-d\right)^2}=\frac{IW^2}{2Rd},$$

or $ML^2 - LN^2 = IW^2$,

or $MI^2 - IN^2 = IW^2$,

or $MW^2 = IN^2$,

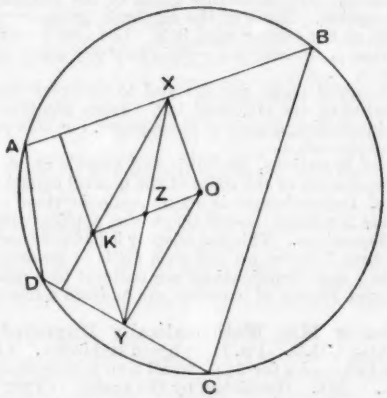
or $IN = \frac{R}{2} - r$, as, of course, $WD = r$.

Similarly $I_1N = \frac{R}{2} + r_1$. Q.E.D

It is scarcely possible that such an elementary proof is new, but I am not aware of any proof on these lines.

Brighton Grammar School. J. PEACOCK.

885. [K¹. 8. b.] *The perpendiculars drawn to the sides of a cyclic quadrangle from the mid points of the opposite sides are concurrent.*



Let O be the centre of the circumscribing circle, and X, Y the mid points of two opposite sides AB, CD.

Let the perpendiculars from X to CD and from Y to AB meet in K.

Then $OXKY$ is a parallelogram, with diagonals bisecting each other at Z, say.

Hence K is the image of O in the point Z.

But Z is the centroid of the *four* vertices A, B, C, D, and will therefore reappear in the same connection if we repeat the construction with either of the remaining pairs of sides.

Hence all six perpendiculars go through K. H. LOB.

---

478. We visited the Jesuites Church. Here Father Kircher (Professor of Mathematics and the Oriental Tongues) shew'd us many singular courtesies, leading us . . . into his own study, where, with Dutch patience, he shew'd us his perpetual motions, catoptrics, magnetical experiments, models, and a thousand other crotchets and devices, most of them since published by himself, or his industrious scholar Schotti.

Nov. 23rd. I went to the Jesuites College againe. . . . Here I heard Father Athanasius Kircher upon a part of Euclid which he expounded.—Evelyn's *Diary*, Rome, Nov. 8, 1644.

REVIEWS.

Elementary Algebra. Part II. By F. BOWMAN. Pp. 431. 6s. (Longmans, Green.)

This second volume is more attractive than the first. It opens with what is, from one point of view, a good introduction to Convergence; no theory of irrationals is included, and so the treatment is necessarily provisional and incomplete, but the author makes this point clear.

The next chapters deal in turn with the Binomial, Exponential, and Logarithmic series; each series is obtained by means of assumptions about the possibility and uniqueness of expansions and the legitimacy of term-by-term differentiation, but here again the assumptions are stated. Deductions of the series, "by strictly algebraic means," are given at the ends of the chapters. Is this necessary? Mathematics is not now divided, for teaching purposes, into water-tight compartments, and the merit of providing strictly algebraic proof is questionable. The deduction given of the binomial series requires yet another assumption. Some of the algebraic geometry might have been omitted, although, as the author says, it is "included to make the book self-contained." Actually, students are certain to study algebraic geometry in an independent text-book.

More than a hundred pages are devoted to Complex Numbers and their developments, including the theorem that "every algebraic equation has a root." There is interesting matter in these pages, but this part of the subject is probably over-represented.

In the Theory of Equations, the cubic and quartic are considered in some detail, as is the separation of the roots of the general equation.

The account of Determinants is more readable than many elementary accounts, and there is a much needed chapter on applications of Determinants to Simultaneous Equations. This last chapter is however hardly as interesting as it might have been if more use had been made of geometrical illustrations.

"Arrangements" and Combinations are reduced to a minimum, and Continued Fractions and Theory of Numbers are omitted altogether. A. R.

The Evolution of Man Mathematically Disproved. By the Rev. WILLIAM A. WILLIAMS, D.D. Pp. 125 + hymn and index. Price $1.00, mailed to all countries; two copies for $1.00 to all who will become agents at 50 per cent. commission. 1925. (Published by the author at 1202 Atlantic Avenue, Camden, New Jersey, U.S.A.)

This book will perhaps be sufficiently described by quoting a short extract from it:

"Now, if there had been no flood to destroy the human race, then the descendants of Adam, in the 7333 years, would have been 16,384 times the 1,804,187,000, or 29,559,799,808,000; or computed at the Jewish rate of net increase for 7333 years since Adam the population would have been still greater, or 35,184,372,088,832. These calculations are in perfect accord with the Scripture story of the special creation of man, and the destruction of the race by a flood. Had it not been for the flood, the earth could not have sustained the descendants of Adam. Is not this a demonstration, decisive and final?"

Those readers of *The Mathematical Gazette* to whom this decisive and final demonstration carries conviction are cordially recommended to drink deeper from the fountain of wisdom at the relatively trivial cost of $1.00, or to become agents at 50 per cent. commission. W. HOPE-JONES.

Wave Mechanics: An Introductory Sketch. By H. F. BIGGS. Pp. 77. 4s. 6d. 1927. (Oxford University Press.)

During the last two years the subject of quantum mechanics has developed with a rapidity which is nothing short of phenomenal. Two years ago it seemed impossible to develop a rational theory of dispersion or to make a direct calculation of the intensities of spectral lines. It is true that the Corre-

spondence Principle had provided a good working rule, but there were difficulties which could not be resolved in terms of the accepted quantum conditions. These had to be supplemented by a number of *ad hoc* selection rules, whose theoretical significance was only partly understood. It was clear that the quantum theory, in spite of its astounding successes in the theory of spectroscopy, was by no means final and a new point of view was wanted.

The critical step, which has led to such enormous developments, was taken by Heisenberg in 1925. Instead of concentrating on the co-ordinates and velocities (or momenta) of electrons in atoms, Heisenberg showed that it was more rational to consider only quantities which were directly amenable to observation, such as emitted frequencies, intensities, etc. This change of outlook met with immediate success, for it led in the hands of Born and Jordan in Germany and Dirac in England to a more rational system of quantum mechanics with a simplification of the essential quantum conditions. From the physical point of view the development was not so satisfactory, as it led to the disappearance of those mental pictures of the atom, such as electrons revolving in prescribed orbits, which had become so familiar.

Following on the heels of these developments came a series of brilliant investigations by Schrödinger with a new point of view. Schrödinger re-emphasised the close analogy, already pointed out nearly a century ago by Hamilton, between ordinary dynamics and geometrical optics. But geometrical optics fails to deal with interference and diffraction phenomena, and has had to be replaced by a more general *wave* optics. In a similar way ordinary Newtonian (or Hamiltonian) dynamics fails to deal with atomic phenomena, and the inference is that it also must be replaced by something more general—a *wave* dynamics. How this was done by Schrödinger, and what measure of success he achieved, are briefly and lucidly told in the short monograph by Mr. Biggs.

Mr. Biggs has attempted to give us "Wave Mechanics without Tears." Whether or not his readers will deem him to have been successful will depend on their mathematical attainments before they begin. However, they may be tolerably certain that no shorter cut is likely to be found, and, though they may be baffled here and there by details owing to the essential mathematical difficulties of the subject, they may yet gain some understanding of the general scheme.

In Schrödinger's theory material particles are replaced by wave systems, so that now we have a dual interpretation of matter just as for many years we have had a dual interpretation of radiation. We have to recognise the value of representing material particles by wave systems just as we have accepted the necessity of representing radiation by a stream of light quanta.

The mastery of this little book will doubtless whet the reader's appetite for more, and he will be stimulated to appeal to the original papers for a knowledge of further developments. He will be prepared to learn of the blending of the theories of Heisenberg and Schrödinger into one unified mathematical whole, though the physical interpretations remain distinct. He will be ready for the extensions of the theory by Born to solve the problem of the interaction of atoms with electrons in collision and for the researches of Dirac on the interaction of atoms with a field of radiation. He will, in short, have been brought within measurable distance of the existing boundaries of knowledge in the realm of theoretical physics. J. E. LENNARD-JONES.

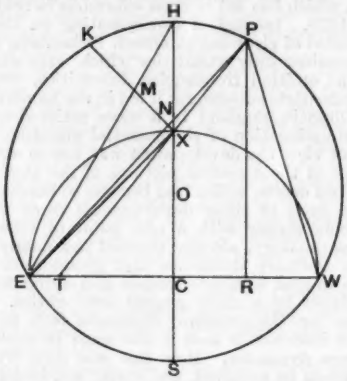
Trisection of an Angle. By W. B. STEVENS. Pp. 5. n.p. 1926. (Warne, Truro Road, St. Austell.)

A series of Euclidean constructions and figures claiming, apparently, to effect the general Geometrical Trisection of the Angle. The author attempts no sort of proof, and we infer that for this he relies on *measurement* entirely.

As pointed out by De Morgan many years ago, measurement, by itself, cannot be accepted as final in this connection; and the result in this case is unfair both to the reader and to whatever merit the construction itself can boast. Indeed, a problem as old as the potter's wheel does seem to deserve something in the way of demonstration rather more ceremonious than the bald assertion "then it will be found that (*e.g.*) the angle CTX equals three

times the angle RPW". Perhaps some day all Euclidean proofs will be reduced to the comfortable words, "So that's that!" But the time is not yet.

Under the circumstances the only course open to the reviewer is to attempt the proof or disproof of one or more of the problems. Time is limited, and as Problem I seems fairly typical, we will proceed to examine it.



Stripped of all superfluous detail it amounts to the following:

In a circle EHW (centre O) let EW be a side of the inscribed equilateral triangle. Bisect EW in C, and on it describe a semicircle EXW. Draw the straight line $SCOXH$ through the centres.

Then taking any point T in EC, joining TX and producing it to cut the circle in P, we draw PR perpendicular to EW, and join PW.

"Then it will be found that the angle CTX equals three times the angle RPW," and that is all the author does to satisfy us; so proceeding by ourselves we join EH, EP and EX, and draw XK at right angles to EX, cutting EH and EP in M and N respectively.

Now $$\hat{CTX} = 45° + \hat{EXT} = 45° + \hat{PEX} + \hat{EPX} \quad \text{.....(1)}$$

and $$\hat{PER} = 45° + \hat{PEX}; \quad \therefore \ \hat{EPR} = 45° - \hat{PEX}$$

and $$\hat{RPW} = 60° - \hat{EPR} = 15° + \hat{PEX};$$

$$\therefore \ 3\hat{RPW} = 45° + 3\hat{PEX}. \quad \text{.....(2)}$$

So that if the author's claim is correct, we get from (1) and (2) that

$$\hat{EPX} = 2 \,.\, \hat{PEX}$$

from which it would follow that

$$XP = \tfrac{1}{2}EN,$$

and EN is less than EM, and it is easily shown that $XH = \frac{1}{2} \,.\, EM$, so that it would follow that

$$XP \text{ is less than } XH.$$

But (Euclid, III. 7) XP is greater than XH; and therefore $\hat{EPX}$ does not equal $2 \,.\, \hat{PEX}$, and consequently the whole claim falls to the ground and the Trisection fails except in the two extreme cases when T coincides with E or C.

But it must be admitted that the discrepancy between $\hat{RPW}$ and $\frac{1}{3}\hat{CTX}$ is very slight indeed; and, if the author only claimed an *approximate* trisection, we should be very ready to congratulate him on a happy effort.

In conclusion we would add that the printing, figures, and whole "get up" of the pamphlet are beyond criticism.

C. H. CHEPMELL.

The Foundations of Euclidean Geometry. By H. G. FORDER. Pp. xii + 349. 25s. 1927. (Cam. Univ. Press.)

At the beginning of the present century two streams were sweeping in opposite directions those engaged in the teaching of the elements of Geometry. In this country teachers were mostly affected by that movement which came to be known in the Press as "the dethroning of Euclid." As the result of this, in the teaching of young pupils, we have gained freedom to modify Euclid's sequence, and freedom to make use of what these pupils suppose to be intuitively true as to the nature of the space in which we live. Thus the pupil who does not eventually become a mathematical specialist learns little or nothing about Axioms and Postulates, nor is he worried about the definitions of such fundamental entities as point, straight line, and plane. The other movement showed itself chiefly in Italy and was associated primarily with the school of Veronese. This aimed at greater, not less, rigour in the initial presentation of the subject. For instance, Euclid's definition of a straight line was to be abolished, not because it could be replaced by an intuitive idea, but because to "lie evenly between" is a phrase of no exact meaning and requires much previous investigation. There must be a more careful and logical step from the concept of a point to that of a line. The final report of the National (U.S.A.) Committee on the Geometry Syllabus (1912) says in this connection: "This recent Italian emphasis upon extreme rigor has led to deplorable results with the less gifted pupils, and a reaction appears to be setting in." Teachers will agree that any attempt to be too rigorous with young pupils must fail. This Italian experiment was inspired by the advanced research then being carried out in various countries which aimed at laying afresh the foundations of the processes of mathematics by discovering, as far as can be reached, the ultimate bases on which all conceptions of number- and space-relations depend. This work is associated with the names, among others, of Peano, Hilbert, Veblen, Russell, and Whitehead. The work in this field dealing with number-concepts is probably more familiar to most English readers than that dealing with the concepts of Geometry. The most widely known of papers on the latter is perhaps that by Veblen on "The Foundations of Geometry" in *Monographs on Modern Mathematics* (J. W. A. Young, 1911). This contains the following footnote: "A book giving a complete and rigorous treatment of elementary geometry would be a most important influence in improving the teaching of the most ancient and perfect of sciences. Such a book could rarely, if ever, be used in the class-room, but if it were in the hands of the teachers it would serve to keep before them in something like its actual form the structure of which they are trying to give their students a first glimpse."

Mr. Forder's book is just such as is indicated in this footnote. This aim is made clear in his preface:

"Part of this book deals with the elementary geometry taught in schools and it might be thought that when the foundations had been laid, we could refer to the school-texts for this portion of the work; and this I should have done, but for the fact that scarcely one proof in any school-test will survive a critical examination, even from the point of view of the writers, still less from ours. . . . This book is clearly not intended for beginners; but the teachers of elementary Geometry and the writers of elementary text-books can learn from it how far short of logical perfection are the proofs usually received; and this should result in an improvement in Geometrical teaching, unless indeed it be contended that an unsound proof has an educational value not possessed by a sound one. Geometry, in its first stages, is rightly taught empirically, and all propositions, regarded by the unsophisticated mind as obvious, are rightly assumed; and while the content of mathematical knowledge is being increased, demonstration can be divorced from experience and intuition only at great sacrifice. But when some well-defined field has been covered, minds of a critical and logical bent will wish to derive logically the propositions in that field from as small a number of assumptions as possible. At this stage the utmost rigour and abstractness are desirable: and this stage can be reached by some in the later years of school life. As Professor G. H. Hardy has recently said: 'Anyone who has the faintest hope of a scholarship

at Oxford or Cambridge could learn the nature of an axiom and how a system of axioms may be shown to be consistent with, or independent of, one another.' "

In the light of the text of the book under review it is worth consideration to what extent the author justifies his wholesale condemnation of current text-books on geometry, and also what teachers can learn from it about the methods of presentation of the groundwork of the subject. Let it be said at once that Mr. Forder's book is one of very high value. It should find a place in every mathematical and teachers' library. The author bases some of his work on recent research by German and Italian mathematicians, which is thus available in English for the first time. Considering the difficulties of expression of all work in this field, Mr. Forder sets out the arguments with clarity; the book is attractive to handle and read, and it is singularly free from the usual misprints. In Chapter I. we have a general introduction with a discussion of such terms as class, relation, ordered couple, the sign of equality, and the meaning of Axiom. Here the author gives in a short paragraph the formalist view of any branch of mathematics: "In our deductions it is not necessary, though it may be helpful, to have in mind any interpretation. We may pretend that we are just playing a game, deducing in accordance with logical laws propositions containing terms which do not have assigned definite meanings, from Axioms of the same sort." The remainder of the chapter contains a résumé of non-geometrical work needed in the course of the subsequent chapters, *e.g.* the fundamental definitions and laws of Algebra. In the second chapter we start "slowly and warily" with the development of geometry. At the outset we are warned that figures will be dispensed with as much as possible. When employed, their sole function will be to help the reader to follow the reasoning; in the reasoning itself they must play no part. The work here is an amplification of that of Veblen referred to above. Starting with the concepts of 'point' and 'order' we are led through six Axioms of order to the idea of a straight line as a set of points satisfying these axioms, and, through two other axioms, to the ideas of space and plane. In the next chapter we are led on to the ideas of regions in a plane, angles and polygons. It has become clear, by this time, what the author means by moving "slowly and warily." The ordinary elementary text-book assumes without question the existence of lines and points which can be "drawn" or "shown" in a figure, and that the order of points on a line must be just as they "appear" in a figure, not always drawn after due consideration of all the possibilities. In the exact demonstration of this book, we are confronted with existence theorems at every step, and considerations of order are of primary importance. It is also made clear that a geometry is a body of propositions which arises from a set of consistent axioms. So far, our axioms apply not only to Euclidean space but to other spaces as well. Every new axiom which is introduced will further limit the spaces to which the propositions apply. The ultimate aim is to base those propositions which apply to Euclidean space alone on as few and as simple a set of axioms as possible. In Chapter IV. we have a definition of congruence expressed primarily, not by a comparison of "measures"—for measure is not yet defined—but as a one-to-one correspondence between points. There are seven axioms of congruence, and four "constructions"—corresponding to Euclid's Postulates. From the point of view of the teacher, this is the most important part of the book, and it is the most difficult to follow. Dismissing the method of superposition in a contemptuous paragraph, Mr. Forder proceeds to develop the argument with the utmost care. His axioms are weaker than those of Veblen or Hilbert, and the demonstrations therefore longer. The frequency of references to preceding paragraphs is, here, rather bewildering, and although the general line of argument is easily grasped, to follow all the details requires concentrated and continuous reading. With regard to angles, the author points out that Euclid assumes implicitly that angles are a set of magnitudes, except that the sum of two angles need not be an angle. Mr. Forder avoids this difficulty by defining "angular measure" as an ordered couple (n, σ) where n is a natural number and σ is the measure of an angle as previously defined, or 0. This is the logical analogue of n half-turns plus a turn through an angle σ, though the logical development

is, of course, independent of this intuitive idea. This device ingeniously avoids difficulties such as reflex angles, but it is confusing to have the terms "measure of an angle" and "angular measure" with different significations. Chapter V. is on circles and spheres, and in VI. we reach a parallel axiom. This is given in its usual form (Playfair's axiom), and it is then shown that a much weaker form will suffice if taken in conjunction with the previous axioms of order and congruence. The usual properties connected with parallels are deduced and are followed by the theorems of Pappus and Desargues and the axioms of Projective Geometry. In defining proportion (Chapter VII.), Hilbert is followed, *i.e.* if OST and $OS'T'$ are two straight lines, OS is to OS' as OT is to OT' is taken to mean that SS' and TT' are parallel. In subsequent chapters there follow an algebra of points and analytical geometry, based on previous axioms, studies of ruler and compasses constructions and of the dissection of plane polygons, and a chapter on areas of polygons and volumes of polyhedra. All throughout, the author has been aiming at weakening his axioms as much as possible, in order to discover the smallest logical base on which the fabric of Euclidean Geometry can be constructed. In the opening paragraph of Chapter XIII. he incites the reader to share in his excitement as he reaches to the goal of his labours. He introduces a continuity axiom, so far strangely omitted, as it has seemed, and then presents us with the result that the whole of Euclidean Geometry can be made to rest on the eight axioms of order, a weakened parallel axiom, and an axiom of continuity. Propositions which do not rest on the parallel axiom are true also for Hyperbolic Geometry.

As indicated by this analysis this book is of great importance and interest to all interested in the development of mathematical logic. The question, however, remains to what degree the book will be of enlightenment to that large body of teachers for whom it was, in part, designed. For the majority of those engaged in the teaching of elementary mathematics are not trained in the methods and manners of thought of this exact mathematical logic, nor have they time to give to the study of a book which requires concentrated and consecutive reading. Yet they are told that with their more gifted pupils they should, after the two preliminary stages (Experimental and Deductive), attempt a third, systematising, stage, the aim of which should be "to arrange the theorems in a logical sequence depending on a comparatively small number of axioms. The reasoning must be rigorous, relatively to the mental age of the pupils" (*M.A. Report on The Teaching of Geometry in Schools*, p. 16). It is probable that in most cases this systematising stage is imperfectly done as regards descriptive geometry, and that the teacher, uncertain himself as to a body of consistent axioms which will go some way towards satisfying modern rigour and yet will be possible in relation to the mental age of the pupils, pushes the question aside, and takes his pupils on to projective geometry in which the logical background is easier to perceive and greater precision in expression and statement easier to obtain. But many teachers feel unhappy about the situation. Such will turn to this book for help. They will certainly learn what an ideal proof should be. It will be gathered, for instance, that the logical weakness of elementary teaching is the reliance upon a figure; whereas in an ideal proof care should be taken to investigate whether the order of points on a line or of rays proceeding from a point can or cannot logically be otherwise than is indicated by a figure, drawn as common sense suggests. As a minor point, readers should be impressed by the value of the line-pair or cross as a means of organising certain groups of theorems. Remembering the criticism in the preface about the fallaciousness of nearly all text-book proofs, the reader will turn to Mr. Forder's corrections of these. In most cases these corrections are concerned with questions of order. Thus in the proof that a quadrilateral $ABCD$ in which AB, CD are equal and BC, AD are equal must be a parallelogram, the case in which B and D lie on the same side of AC, D lying within the angle ABC, is generally passed over without comment. In the earlier stages this is rightly so. Even if quadrilateral has not been defined to mean convex quadrilateral, the pupil knows that he is only proving the converse of the parallelogram property and that it "wouldn't be sensible" to draw the figure the other way. This is exactly the sort of case which can

be investigated at the systematising stage. Another of the author's valid criticisms is directed against a proof, commonly found, of the theorem: if B and C lie on the same side of AD and angles ABD, ACD are equal, then $ABCD$ are concyclic. The proof consists of saying, draw the circle through ABD and let it cut AC in E, then by *reductio ad absurdum*. The possibility that AC might be a tangent to the circle is generally overlooked. But a study of Mr. Forder's corrected proofs does not, on the whole, bear out the contention in his preface that "scarcely one proof in any school text will survive a critical examination from the point of view of the writers." There are carelessly written text-books in geometry as in every other branch of learning, but the writers of most geometrical books are fully aware of the logical imperfections of their system of demonstration. Here is an extract from the introduction to the more formal portion of a widely used text: "In Theoretical Geometry the ideal is that each theorem shall be deduced from previous theorems or from explicit assumptions. . . . Formerly it was thought that Euclid had reduced the science of geometry to its simplest assumptions, but it is now known that certain other fundamental assumptions must be made which are not stated in Euclid's work. For this reason we do not attempt to enumerate the root assumptions of the subject, some of which indeed are very difficult to understand. To this extent we fall short of the ideal described above." It is precisely because no explicit statement of the root assumptions is made in an elementary text-book that the words "critical examination from the point of view of the writers" have no exact meaning. And most of Mr. Forder's corrections of the traditional proofs will leave the writers of text-books unrepentant. For instance, in a special footnote he says that in a great many school texts the proofs of the simple propositions on tangency to circles are fallacious. His corrected proofs involve as theorems in the chain: (i) A line which has a point P on, and a point Q inside a circle, centre O, has one other point on the circle; (ii) if PX touch a circle at P, no point of PX is inside the circle; (iii) if PX meet a circle, centre O, at P, and no point of PX is inside the circle, then PX is a tangent at P. These are not explicitly stated in an elementary text-book, but are a part of the unexpressed root assumptions accepted as intuitively true.

The situation then comes to this. The pupil who reaches Open Scholarship standard does not usually, at school, investigate the foundations of geometry with any completeness. It may be added that the schoolboy never did, for the Euclidean axioms which satisfied previous generations do not satisfy us. Professor Hardy and others say that some of this work should be done at the school stage. The answer of the teacher is, firstly, the difficulty of finding time for such work, taking into account the demands now made that work in analysis should be done with much greater care and completeness than formerly. Secondly, if such time can be found, what kind of research into foundations will be practicable and satisfactory? Mr. Forder admits that his type of book is not for the schoolboy. Is there a half-way house? Is it possible to write a book that will go some way, but not the whole way, towards satisfying the demands of modern rigour? In other words, is it possible to state a body of axioms and assumptions, not the "weakest" possible, and therefore redundant, which can be shown to be sufficient and consistent, and on which the fabric of geometry can be erected? If this could be done, and formed into a book of moderate proportions, it might do a great deal to improve the teaching of "the most ancient and perfect of sciences." There would then remain the further question whether this work should be done at the school stage or whether time is not better spent in deductive geometry and in analysis. But of that *quot homines tot sententiae*.

H. E. PIGGOTT.

Mathematical Investigation in the Theory of Value and Prices. By IRVING FISHER. Pp. xii, 126. 4s. 6d. (Yale University Press.)

Professor Fisher has reprinted his Doctor's Thesis, published more than thirty years ago. The thesis is now of interest, not so much for the mathematical representation of economic theory, as for the description of the apparatus that he designed to compare economic and mechanical forces.

This apparatus consists of a large tank in which are suspended smaller

tanks. The heights of these tanks, and the heights and quantities of the liquid they contain, represent the utilities, prices, outputs, etc., of commodities. A variation in one of the factors of the apparatus will affect the remainder of the mechanism in a manner similar to the changes produced in the business or economic world by an alteration in the supply of or demand for a single commodity.

This ingenious toy may help the economic student to realise how the values of different goods are interdependent. The assistance it gives differs from that rendered by graphs or by pure mathematics.

Graphs enable the student to grasp more easily such ideas as rent and consumer's surplus. And graphical notation can illustrate and make clear the answer to such a question as: "Is a tariff on an imported article, also produced in the taxing country under conditions of increasing return, advantageous to this country?"

The representation of economic functions by mathematical symbols has occasionally been helpful: but the usefulness of mathematics to the economic student lies rather in its provision of a mental training which enables him easily to follow economic arguments.

Professor Fisher's book will probably appeal more for the originality of his apparatus than for the mathematics. E. F. S.

Etude Elémentaire de l'Hyperbole Equilatère et de quelques Courbes Dérivées. By J. LEMAIRE. Pp. iv + 172. 12 fr. 1927. (Vuibert.)

Twenty years ago there appeared in the *Gazette* over a name long since forgotten an article entitled "A New Cubic connected with the Triangle", which drew from the late R. F. Davis an expostulation that the cubic, far from being new, had perpetually haunted the meetings of the M.A. in the days when the geometry of the triangle was a vivid delight. The curve was in fact the strophoid, and Davis was not hinting at its real age or celebrity, for it was known to Barrow, and was the subject already by 1860 of so many memoirs that Tortolini thought it worth while to publish a list that year in his *Annali*. Systematic accounts of the strophoid are of course to be found in the encyclopaedic treatises of Loria and Teixeira on special curves, and there are a few pages on it in Wieleitner's book, but English books on plane curves, and even English books on the calculus, have only casual references.

The strophoid not only exhibits the many projective properties of the nodal cubics, but since it is the inverse of a rectangular hyperbola through the centre of inversion it is peculiarly rich in simple metrical properties also. For example, if A, B, C are fixed points, the locus of P if PA is an axis of the cross (PB, PC), or, in other words, if PA is a bisector, internal or external, of the angle BPC*, is a strophoid which has A for node and passes through B and C. Apart from scale and orientation, the strophoids with A for node form a singly infinite family, and the triangles with A for one vertex form a doubly infinite family; we therefore anticipate that the strophoid plays the same part as a locus for an infinity of its chords, and in verifying this result we find one among a number of useful pairings of the points of the curve.

Although a few sections are devoted to the lemniscate, M. Lemaire's unpretentious book is really an account of the strophoid, preceded by a discussion of the metrical properties of the rectangular hyperbola which invert without complication, and in particular of properties of groups of concyclic points on the curve, and followed by some theorems deduced by projection and reciprocation, which are theorems on the general nodal cubic and on the three-cusped hypocycloid. The work is admirably planned and well written; dealing with a narrow subject, the author is remarkably successful not merely in avoiding

* It is often said that AB and AC subtend equal angles at P; in this form the condition is too narrow, for it excludes the arc BAC of the curve, and too wide, for it admits the parts of the line BC beyond B and C. To include the whole curve some writers say that the angles subtended at P are to be equal or supplementary, but then it is necessary explicitly to remove the whole of the line BC. We may describe the locus by saying that either the angles APB, CPA are equal or the angles APB, APC are supplementary, but surely it is better to learn so emphatic a lesson in the convenience of using crosses. Perhaps the closest accurate approach to the primitive point of view is to say that the crosses (PA, PB) and (PC, PA) are congruent.

trivialities but in conveying a sense of a wide outlook. There are clear and abundant diagrams, and a small collection of examples. Even the teacher who is afraid to extend his geometrical range beyond the conic will be stimulated by the first half of the book, while any reader with a taste for metrical pure geometry will find the book fascinating—from cover to cover, I was about to say, but unfortunately the phrase is not apposite ; if one enterprising French publisher pandered to our weakness by issuing bound editions for the English market, would our demand encourage others to follow suit ?

E. H. N.

Lehrbuch der Ballistik. By DR. C. CRANZ. Vol. II. Innere Ballistik. Pp. x+454. 37 gm. 1926. (Springer, Berlin.)

Internal ballistics is a subject which has gradually developed since the invention of gunpowder ; it appears, however, to have been three hundred years after this invention that the first quantitative experiments were made on the action of gunpowder in a gun. The results of these experiments, which were conducted by Robins, were published in 1743. Since that date the subject has attracted chemists, physicists and mathematicians in many countries, with the result that an abundance of experimental and theoretical literature is available. In compiling this volume on internal ballistics Dr. Cranz and his collaborators, Professors O. Poppenberg and O. von Eberhard, have therefore been faced with the difficult problem of selection. That they have succeeded in giving a clear and progressive account of the subject while indicating the results of many investigators testifies to their success in solving this problem.

An account is first given of the characteristic properties of explosives, and experimental results of burning in a closed vessel (*i.e.* a strong vessel of constant capacity) are analysed and discussed. Two methods are given of calculating the temperature of combustion, one from the heat content, the other from the maximum pressure exerted in the closed vessel. Experimental methods of measuring high pressure are next reviewed and discussed, particular attention being given to the errors of crusher gauges.

Combustion, explosion and detonation are considered, and an attempt is made to differentiate between them in terms of rate of burning. Various experimental laws for rate of burning as a function of pressure are given, together with an account of the geometrical deformation of the pieces. Then follows an interesting chapter on the mechanics of explosion dealing with the propagation of the pressure wave (Lagrange's problem) and erosion of the bore.

The principal problem of internal ballistics—the calculation of gas pressure and velocity of projectile in the bore—is ably treated, various methods of solution being reviewed and discussed, including a new graphical solution, by Cranz, for a rate of burning proportional to the pressure.

An account of the mechanics of recoiling mechanisms is followed by a chapter on explosive shell in which the rate of burning of time fuzes is considered. The book concludes with some excellent photographs of a bullet emerging from the muzzle of a rifle, showing the development of the explosion wave.

A comprehensive bibliography and a large number of references to papers, monographs, etc., form a useful appendix to this book, which is a valuable addition to the literature of the subject.

F. R. W. HUNT.

A MEMBER of the Association would like to purchase *Proceedings of the Edinburgh Mathematical Society*, Vol. XI.—Apply to G. M. R., 68 Victoria Street, Westminster, S.W. 1.

PRINTED IN GREAT BRITAIN BY ROBERT MACLEHOSE AND CO. LTD.
THE UNIVERSITY PRESS, GLASGOW.

CAMBRIDGE BOOKS

Recently Published

A TREATISE ON THE ANALYTICAL DYNAMICS OF PARTICLES AND RIGID BODIES, with an Introduction to the Problem of Three Bodies. By E. T. WHITTAKER, F.R.S. Third edition. Royal 8vo. 25s net.

Chapters I-XIV of this edition have been reproduced with some corrections and additional references. Chapters XV (The General Theory of Orbits) and XVI (Integration by Series) have been completely rewritten in order to present the subject as it has been developed by the researches of the last eleven years.

THE THEORY AND FUNCTIONS OF A REAL VARIABLE AND THE THEORY OF FOURIER'S SERIES. By E. W. HOBSON, Sc.D., LL.D., F.R.S.

Volume I. Third edition, revised throughout and enlarged. Royal 8vo. 45s net.

CALCULUS OF VARIATIONS. By A. R. FORSYTH, Sc.D., LL.D., Math.D., F.R.S. Royal 8vo. 50s net.

CREMONA TRANSFORMATIONS IN PLANE AND SPACE. By HILDA P. HUDSON, O.B.E., M.A., Sc.D., A.F.R.Ae.S. Royal 8vo. 42s net.

ELEMENTARY DIFFERENTIAL CALCULUS. By G. L. PARSONS, M.A. Crown 8vo. 6s.

FOUR NEW

Cambridge Tracts in Mathematics and Mathematical Physics. General Editors: G. H. HARDY and E. CUNNINGHAM.

No. 21. **THE THEORY OF INTEGRATION.** By L. C. YOUNG. Demy 8vo. 6s net.

No. 22. **INTEGRAL BASES.** By W. E. H. BERWICK, Sc.D. Demy 8vo. 6s 6d net.

No. 23. **OPERATIONAL METHODS IN MATHEMATICAL PHYSICS.** By HAROLD JEFFREYS. Demy 8vo. 6s 6d net.

No. 24. **INVARIANTS OF QUADRATIC DIFFERENTIAL FORMS.** By O. VEBLEN. Demy 8vo. 6s 6d net.

CAMBRIDGE UNIVERSITY PRESS

Fetter Lane, London, E.C. 4

THE MATHEMATICAL ASSOCIATION.

(An Association of Teachers and Students of Elementary Mathematics.)

"I hold every man a debtor to his profession; from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavour themselves, by way of amends, to be a help and an ornament thereunto."—BACON (Preface, *Maxims of Law*).

President:

Prof. M. J. M. HILL, LL.D., Sc.D., F.R.S.

Vice-Presidents:

Prof. G. H. BRYAN, Sc.D., F.R.S.
Prof. A. R. FORSYTH, Sc.D., LL.D., F.R.S.
Prof. R. W. GENESE, M.A.
Prof. G. H. HARDY, M.A., F.R.S.
Sir T. L. HEATH, K.C.B., K.C.V.O., D.Sc., F.R.S.
Prof. E. W. HOBSON, Sc.D., F.R.S.
A. LODGE, M.A.
Prof. T. P. NUNN, M.A., D.Sc.
A. W. SIDDONS, M.A.
Prof. H. H. TURNER, D.Sc., D.C.L., F.R.S.
Prof. A. N. WHITEHEAD, M.A., Sc.D., F.R.S.
Prof. E. T. WHITTAKER, M.A., Sc.D., F.R.S.
Rev. Canon J. M. WILSON, D.D.

Hon. Treasurer:

F. W. HILL, M.A., 9 Avenue Crescent, Acton, London, W. 3.

Hon. Secretaries:

C. PENDLEBURY, M.A., 39 Burlington Road, Chiswick, London, W. 4.
Miss M. PUNNETT, B.A., The London Day Training College, Southampton Row, London, W.C. 1.

Hon. Secretary of the General Teaching Committee:

ALAN ROBSON, M.A., The College, Marlborough, Wilts.

Editor of *The Mathematical Gazette*:

W. J. GREENSTREET, M.A., The Woodlands, Burghfield Common, Reading, Berks.

Hon. Librarian:

Prof. E. H. NEVILLE, M.A., B.Sc., 160 Castle Hill, Reading.

Other Members of the Council:

N. M. GIBBINS, M.A.
Miss M. J. GRIFFITH.
F. G. HALL, M.A.
Miss M. A. HOOKE.
H. K. MARSDEN, M.A.
Prof. W. P. MILNE, M.A., D.Sc.
Prof. H. T. H. PIAGGIO, M.A., D.Sc.
Prof. W. M. ROBERTS, M.A.
W. F. SHEPPARD, Sc.D., LL.M.
Miss D. R. SMITH.
Miss L. M. SWAIN.
C. O. TUCKEY, M.A.

Hon. Secretary of the Examinations Sub-Committee:

W. J. DOBBS, M.A., 12 Colinette Rd., Putney, S.W. 15.

THE MATHEMATICAL ASSOCIATION, which was founded in 1871, as the *Association for the Improvement of Geometrical Teaching*, aims not only at the promotion of its original object, but at bringing within its purview all branches of elementary mathematics.

Its purpose is to form a strong combination of all persons who are interested in promoting good methods of teaching mathematics. The Association has already been largely successful in this direction. It has become a recognised authority in its own department, and is continuing to exert an important influence on methods of examination.

The Annual Meeting of the Association is held in January. Other Meetings are held when desired. At these Meetings papers on elementary mathematics are read and discussed.

Branches of the Association have been formed in London, Bangor, Yorkshire, Bristol, Manchester, Cardiff, the Midlands (Birmingham), New South Wales (Sydney), Queensland (Brisbane), and Victoria (Melbourne). Further information concerning these branches can be obtained from the Honorary Secretaries of the Association.

"**The Mathematical Gazette**" (published by Messrs. G. BELL & SONS, LTD.) is the organ of the Association. It is issued at least six times a year. The price per copy (to non-members) is usually 2s. 6d. each. The *Gazette* contains—

(1) ARTICLES, mainly on subjects within the scope of elementary mathematics;

(2) NOTES, generally with reference to shorter and more elegant methods than those in current text-books;

(3) REVIEWS, written when possible by men of eminence in the subject of which they treat. They deal with the more important English and Foreign publications, and their aim is to dwell on the general development of the subject, as well as upon the part played therein by the book under notice;

(4) QUERIES AND ANSWERS, on mathematical topics of a general character